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NOTES AND COMMENTS.

The Pollination of the Yucca.

MR. A. W. BENNETT, in his article in the March number of Natural Science, refers to the pollination of the Yucca, a well-known liliaceous plant, by a moth (*Pronuba yuccasella*), and those interested in the subject will find some "Further Studies," by William Trelease in the fourth annual report of the Missouri Botanical Garden. Hitherto, only two species, *filamentosa* and *glauca*, have been directly observed under cultivation, and the latter wild in Colorado, and the moth *Pronuba* is the pollinating agent in both. The present report describes further investigations in the field on these and other species.

The flowers of *Yucca baccata*, a widely distributed species, ranging in a variety of forms from southern Colorado into Mexico and California, are frequented by a moth somewhat larger and a little darker in colour than the *P. yuccasella* of the Mississippi Valley and Rocky Mountains, but otherwise indistinguishable from it, as is also the moth of the Gulf region. Like the Eastern representative, it rests within the flower during the day with the head directed towards the base of the stamens, and when depositing the egg, at night, "doubtless backs down between the upper ends of the stamens," piercing the ovary at about its middle in the thinnest part, viz., that between the internal partitions, which is not covered by the lower part of the stamen. "After thus depositing the egg in the ovule of the plant, the females behave as in *Y. filamentosa*, carrying loads of pollen from the anthers and thrusting it into the cavity of the stigma with the proboscis. This open canal is frequented by numbers of a white Thrips, which sometimes penetrate the cells of the ovary and doubt-

less assist fertilisation by scattering the pollen. Dried fruits of the previous season showed the perforations made by the escaping larvæ.

Yucca australis, studied in south-west Texas, shows seeds tunnelled in the manner characteristic of the moth, the pulp being perforated by the escaped larvæ. No flowers were observed, but large fruits, gathered some three weeks after fertilisation, bore none of the constrictions or indentations which so commonly mark the puncture of the moth, a fact which suggests that the eggs are deposited in the upper part near the stigma, as was found to be the case in the large tree yucca, *Y. brevifolia*. The agent in the flowers of the latter is the dark-coloured *Pronuba synthetica*, which is more active through the day than its eastern congener, and, unlike the other known species, slow to take flight.

This apparent disinclination to leave the flowers may, perhaps, be connected with the almost constant occurrence of high winds in the desert, and may restrict cross-fertilisation to flowers of the same plant more closely than in other Yuccas, though there must be frequent flights from plant to plant in quiet weather, and especially at night, when the wind sometimes falls. Moreover, the development of the stigma two days before the stamens of any one flower renders close fertilisation, in the strictest sense, impossible. Owing to the light yellow colour of the pollen, in marked contrast with the smoky tint of the moth, laden females can be seen from a considerable distance.

The female of most species of *Pronuba* seeks a fresh flower wherein to lay her eggs, preferring one in the first night of expansion, doubtless to ensure for her offspring a sufficient food-supply, the younger flowers being less likely to be already overstocked with eggs. This instinct is especially marked in *P. synthetica*, which was never seen to use any but the youngest flowers, while pollen-laden moths were repeatedly noticed forcing their way in the very narrow clefts between the rigid sepals of an opening bud, their flattened form facilitating this. When about to deposit an egg, the female runs to the bottom of the stamens much as *yuccasella* does, makes a rapid, more or less complete, circuit of their bases, and then quickly mounts to the very top of the pistil, and with her short, strong ovipostor cuts through the thin wall into the channel of the style close below the tip of the stigma, holding fast to the pistil meanwhile, the stamens being below her reach. The long extensile oviduct is then passed through the puncture, the egg being laid apparently within the ovary cell. The operation usually takes from two-and-a-half to three minutes longer than in other observed species. Sometimes two or more eggs are laid before the stigma is pollinated, but commonly, after laying each egg, the moth retreats to the bottom of the flower, and then again mounts the pistil till her head is even with the stigma, when she uncoils the large tentacles from their resting-place against her load of pollen and passes them back and forth in the stigmatic chamber with almost the same motion as *yuccasella*. The deposition

of the egg just below the stigma explains the remarkable symmetry of most of the fertilised pistils, contrasting with the constriction or indentation of the Eastern species at the point where the ovary has been punctured.

In another smaller species, *Yucca elata*, observed at Eagle Flat, Texas, *Pronuba yuccasella* was the agent of pollination. When about to deposit an egg, the moth, as in *Y. filamentosa*, runs nervously about within the bottom of the flower, then scrambles to the top of the pistil and backs down between two stamens by a succession of jerks, until her head is about level with the base of the style. Clinging to the pistil, she then punctures the ovary, and the egg is consigned to its place in the manner so well described by Riley for *filamentosa*. Usually each oviposition is followed by pollination, but in a few cases two eggs were laid before pollen was carried to the stigma, and sometimes so disturbed is the moth by the bright light of the observer's lantern, that she will leave a flower altogether without pollination. In one such instance her first act on entering another flower was to thrust her pollen-laden tentacles into the stigma, though it is unusual for this to precede oviposition. On several occasions, when disturbed in laying her egg, the moth ran upon a stamen, shaking it quite violently and making several passes at the anther with her tentacles, as if frightened from one of her occupations only to engage in another. The very distinct Californian *Yucca Whipplei*, as Professor Riley has shown, is pollinated by a distinct *Pronuba*, which he names *P. maculata*, of a general white colour, but mottled with black. In this species the pollen is not loose and powdery, as in Yucca proper, but glutinous, and frequent observation has shown that it may be deposited on the stigma directly from the anthers in closing flowers, so that self-pollination becomes possible, and Mr. Coquillet, of Los Angeles, records the seeding, in 1892, of a number of panicles which he had covered with gauze before any of the flowers opened. Owing to the more open and more diurnal character of the flowers in this species, the moth is far more active in the daytime than its congeners. Moreover, the resting position, with the head towards the stigma, is almost identical with that taken in oviposition, which, like pollination, may be witnessed at any time during the day. Standing on the side of the pistil the female pierces the ovary at the thinnest part. Generally not more than six eggs are laid in one pistil—one on either side of each septum—often fewer, so that even if they all hatch—an unlikely event—there is rarely more than one larva to each tier of seeds, thus allowing a fair percentage to ripen. Pollination was well seen in the variety *graminifolia*, where it was observed several times under a cloudless sky, the first time at about noon. Flying into a flower, the moth runs about the bases of the stamens in the usual way, then quickly clambers upon the inner side of a filament, and, with the tentacles stretched over the pollen masses, drags first one and then the other out of the anther cells, pressing

them together under the throat, and subsequently working them into a compact mass, much as *yuccasella* does the powdery pollen of other yuccas. The stamens are often quite violently shaken, so quick and energetic are the movements of the moth.

As the author suggests, the relationship of the Yuccas to one another and to the Pronuba moths would be far more intelligible could we trace their history, even a short way back, into geological time, but as no certainly identifiable Yuccas exist in even the latest deposits, this is as yet impossible.

He thinks it reasonable to suppose that an ancient type, represented in the tree-like *Y. brevifolia*, with an equally ancient type of Pronuba, has persisted in the Pacific region, perhaps, for the same reasons that have led to the preservation of the gigantic Sequoias in the same region, while the more widely distributed species have become differentiated, and, with their pollinator, passed to the south under new conditions.

THE "ZOOLOGICAL RECORD."

IN the Report of Accounts of the Zoological Society for 1892, there is an item of £937 for "cost of *Zoological Record*." The same entry for 1890 amounted to £650, and for 1891 to £420. There is no statement as to the reason of this enormous increase—a somewhat unfortunate oversight on the part of those who drew up the Report—and it is well that the matter should be explained. The £937 not only includes the *Record* for 1891, but includes a great part of the *Record* for 1890. The "sales" for this invaluable compilation were, in 1890, £298; in 1891, £266; in 1892, £354; a net improvement in the last year of the three of £56.

It seems rather difficult to believe that a volume like the *Zoological Record* should only enjoy the ridiculous sale of barely 250 copies; and it is certainly most praiseworthy that the Zoological Society, despite the annual loss, refuses to allow the good work it is doing to fall to the ground. Surely there is no museum or laboratory in existence that can afford to neglect zoological literature to such an extent as to refuse to subscribe to the *Record*; and yet it is hard to believe that there are only 250 museums or libraries of repute in the whole world. There is certainly no other Record that is anything like so up-to-date as the Zoological Society's publication. It has always been a matter of regret that some amicable understanding has not been arrived at by the Royal, the Linnean, the Geological, and the Zoological Societies which will allow the proper listing and publication annually of a complete literature of the animal, mineral, and vegetable kingdoms. The Zoological Society, despite the enormous outlay required by the Gardens, cheerfully bears the whole of the expense of providing students of zoology with

an efficient yearly catalogue, and sets a worthy example to its sister societies, with their accumulated and accumulating capitals, and the additional benefit enjoyed by them of rent-free premises in Burlington House. We look for better things from scientific societies than heavy investments in the Funds.

Ice in Hong Kong.

The *Gardener's Chronicle* of April 8 gives some particulars of the unprecedented cold weather which occurred in Hong Kong last January. There is no previous record, says Mr. Ford, the Director of the Botanic Gardens, of ice occurring in the island below 1,700 feet, and hence, as it lies within the tropics, the result was most disastrous, indigenous, as well as cultivated, plants suffering.

"The continued low temperature," he says, "combined with a fall of rain from an apparently warmer stratum of air above, resulted in the formation of ice varying in quantity from a thin coating on the upper leaves of pine trees growing at 300 feet above sea-level, to a thick encasement of perfectly transparent solid ice of 5½ inches in circumference on the blades and bents of grass at the summit of Victoria Peak." Leaves of evergreen shrubs and trees bore solid coverings of ice three-eighths of an inch in thickness, the great weight of which caused the branches to assume a pendent form, and in many cases snapped off the limbs. All vegetation throughout the hill regions of the colony was covered in this way with ice, as were also most other objects. Telegraph wires had a casing more than half-an-inch thick, and bore icicles three inches long as close as they could be packed side by side, while the windward sides of the walls of the look-out at the peak were sheeted from top to bottom with a perfectly transparent coat three-quarters of an inch thick. All the hills on the mainland and Lantao Island were likewise white with ice.

The damage in the Gardens consists chiefly in injury to foliage, but some plants, natives of warmer regions, are quite killed, and many of the decorative plants which survived will be months before they reassume their ornamental appearance.

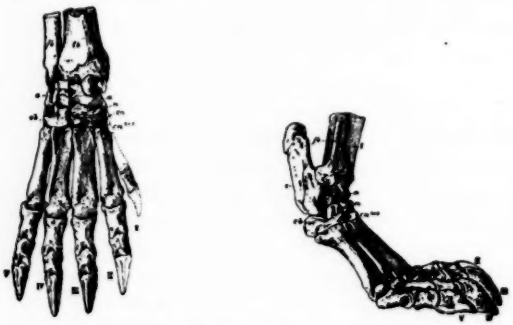
In spite of every possible precaution in matting-in the plant-houses and covering the roofs with straw, the contents suffered greatly, many of the best orchids being killed outright, and others so much injured that even if they survive it will be years before they regain their former luxuriance. It is interesting to note that, while a healthy plant of *Dendrobium aggregatum* received from Calcutta several years ago is apparently killed, plants of the same species growing by its side, and others on trees where they had no shelter, which were collected ten years ago on the Lo-fau mountains, about sixty miles from Canton, have escaped unharmed. Several exotic trees planted on the hills had all their leaves killed, while above 600

feet the indigenous vegetation was much injured, especially the tropical plants which here reach their northern limit.

Dr. Henry reports that the destruction of vegetation about Canton has also been very great. The banana plantations are ruined, the bamboos have suffered, the candle-nut trees (*Aleurites triloba*) are all shrivelled up, while begonias, euphorbias, crotons, and scores of others, are quite destroyed.

A Clawed Artiodactyle-like Mammal.

From several references to the subject, readers of Natural Science are probably familiar with the fact that the remarkable clawed Miocene and Pliocene mammal known as *Chalicotherium* presents many curious resemblances to the ordinary Perissodactyle Ungulates. Messrs. Osborn and Wortman now announce the discovery of another American Tertiary mammal, for which they propose the name *Artionyx*, with almost precisely similar resemblances to the Artiodactyle divi-



Front and hind foot of *Artionyx*.—(From Osborn.)

sion of the Ungulates. The authors refer both these curious creatures to a single group, which they suggest originated from the primitive Ungulates before the acquisition of distinctly Ungulate terminal phalanges; and they call attention to the curious double parallelism existing between these two forms on the one hand and the Artiodactyle and Perissodactyle Ungulates on the other. All this confirms the view which we have so frequently urged as to the impropriety of separating *Chalicotherium* as an order from the Ungulates.

Mountain Chains as Barriers.

Dr. J. Regnault, in a lecture before the Ethnographical section of the Geographical Society of Paris, has drawn attention to the "Influence of Mountain Chains on the Distribution of the Human Race." His original observations resulted from a visit to Darjeeling and the Terai, but his conclusions are drawn from a study of the

whole of Europe and Asia. He states the following laws:—(1) When a chain of mountains, such as the Alps, the Pyrenees, separates two nations, ... one generally thinks that the crest, the water-shed, divides two races, each inhabiting his respective slope up to the summit. Nothing of the kind, and one can say that the crest of a chain does not separate two races. In support of this statement, Regnault says:—This error is due to the fact that the crest generally serves as a frontier between two States. (2) Ordinarily the race of the country towards which the lesser slope inclines inhabits not only this slope, but crosses the crest and colonises the more rapidly-sloping side down to the plain in which the slope is lost. It is the junction between the rapid slope and the plain where the contact of the two races is to be found. To the second law he adds two postulates:—(*a*) The mountain chain must be high and long, as the Alps and Pyrenees; (*b*) The law is modified by considerations of time, geology, climate, &c. Dr. Regnault discusses in some detail the Alps, the Pyrenees, Erz-Gebirge, Caucasus, and the Himalaya. The lecture, which is highly interesting, will be found printed in *Revue de Géographie*, March, 1893.

Myeloxylon.

In the recently-issued number of the *Annals of Botany* (vol. vii., p. 25), A. C. Seward discusses the affinity of the fossil plant genus *Myeloxylon*. Corda thought it a Palm, Brongniart a doubtful Monocotyledon, while Goeppert referred it to those plants which must be looked upon as prototypes or synthetic types, recognising in its structure the Fern, Gymnosperm and Monocotyledon. Binney, Williamson, Renault, and Kidston make it a fern rachis of the order Marattiaceæ, while Schimper, Schenk, and Felix prefer to regard it as the petiole of a Cycad. The present account embodies the result of an examination of some new material from the Millstone Grit, and elsewhere, and also of some specimens now in the British Museum, and once the property of Robert Brown. The author has had the good fortune to get sections showing very well the structure of the vascular bundles, the form of which closely corresponds with that in Cycads of to-day, while the position of the first-formed wood vessels on the side of the wood next to the bast is also in striking agreement with these. The bordered pits, characteristic of the recent Cycads, were not seen, but there are points of resemblance between the wood elements in both, while the resin canals of recent Cycads and the fossil petioles are practically identical.

On the whole, the author concludes that the fossil specimens approach more nearly to the Cycads than the Ferns. The few points of difference which distinguish them from the petioles of recent Cycads are, he thinks, far outweighed by the close parallelism in more essential characters, and he looks upon *Myeloxylon* as an

extinct genus not corresponding exactly to any recent family of plants, but coming very near the Cycads in anatomical structure, and probably holding a position between Cycads and Ferns, but nearer to the former.

EMIN PASHA.

THE *Berliner Tageblatt* for April 5 publishes a letter from its special correspondent, dated Fort Kampala, Uganda, December 14, 1892, which tends to confirm the rumours of the death of Emin Pasha. According to the letter, news had reached Fort Kampala that Emin Pasha marched from Kavalli to Masamboni, and thence to the Ituri river, on the banks of which he was attacked by a body of Manyema and killed. The correspondent of the *Tageblatt* further reports having met an Egyptian official, named Awad Effendi, on his way to the coast, who informed him that he was present with Emin in Masamboni's country, and added that Emin Pasha and all his followers had been murdered on the Ituri river by Manyema, under the leadership of an Arab named Ismail. Awad Effendi believed that the murder was committed on March 12 or 13, 1892.—*Reuter*. This may be another version of the rumour to which we referred in July; at present, we cannot be sure of the accuracy of the information.

THE AFFINITIES OF ZEUGLODON.

IN the last issue of the *Proc. Zool. Soc.*, Mr. Lydekker describes an interesting series of Cetacean remains from the Eocene of the Caucasus. Among these, special importance attaches to certain bones belonging to the imperfectly-known creature designated *Zeuglodon*, which, as our readers are doubtless aware, Professor D'Arcy Thompson has recently endeavoured to remove from its assigned position among the whales to associate it with the seals. The most important specimen among the new find is a humerus, of which but one example has been hitherto known, and the study of this leads the author to conclude that *Zeuglodon* is much more likely to be an ancestral Cetacean than a Seal. Doubtless, however, Professor Thompson will have a word to say on the subject. Scarcely less interesting is the discovery of a skull in the same deposits, indicating a Cetacean nearly allied to the existing *Inia* and *Pontoporia* of the South American rivers. We regret that the artist has scarcely done justice to the author's specimens.

NEW MONKEYS.

IT is probably a unique feature in a single number of the serial just mentioned to find three new species of monkeys described and illustrated with coloured plates. Two of these belong to the Oriental genus *Semnopithecus*, and the third to the Ethiopian *Cercopithecus*.

In regard to one of those belonging to the former genus, the describer observes that specimens may subsequently show a transition from the supposed new form to one which he has previously named *S. hosei*, in which case the former will have to be reduced to the rank of a variety. Nevertheless, he adds, "in the absence of such intermediate forms, and in view of the great constancy in the coloration of *S. hosei* already noted, it seems best to give a name to the striking variation from it now described." We venture to have doubts as to the wisdom of this course.

Pictures at Brighton.

It is not often that Art and Science go hand in hand, therefore two collections of pictures now exhibited on loan at the Brighton Corporation Art Gallery are of an unusually interesting and instructive character, from a scientific point of view. The series of African scenes, lent by Mr. Robert White, painted in oils by Mr. Thomas Baines, the well-known explorer, illustrate the geology, zoology, botany, and ethnology of the South and West regions of that continent. The successive river-terraces of the Great Zambesi, and its winding, deep-cut, cañon-like gorge, are well depicted by the artist, a brother of Mr. Geddes Baines, discoverer of the Dicynodonts. There is a picture of the spot where remains of one of those extraordinary reptiles were first found. Miss C. F. Gordon Cumming exhibits a series of water-colour sketches of Ceylon and Fiji, including a fine set showing the river-terraces, silica-deposits, sulphur, mud, and hot springs of the volcanic district of New Zealand, where the whole aspects of the region were so completely changed by a subsequent convulsion of Nature, that these pictures portray scenes no longer in existence. The luxuriant flora of Ceylon is well illustrated, and there is a capital sketch of the sacred Singalese relic "Buddha's tooth," whether "shed" or "removed" tradition is silent.

Fauna of the Plöner-See.

We are glad to welcome the first number of the *Forschungsberichte aus der Biologischen Station zu Plön* (Berlin: Friedländer), by Otto Zacharias, the Director. The number opens, as it should do, with a list of the Fauna of the great Plöner-See, the compilation of which has brought to light many new forms, all duly described and figured in the succeeding pages. These include a Rhizopod, a Heliozoan, a Mastigophore, an Infusorian, a Turbellarian, and eight Rotifers. If so good a result is achieved in making up a first list, we may expect a large addition to our knowledge of fresh-water life from the work done at the Biological Station at Plön.

THE PEOPLING OF THE WORLD.

THE last of those valuable "Ergänzungshefte" (number 107), published at intervals by the proprietors of *Petermann's Mitteilungen*, is devoted to the "Peopling of the World." Drs. Hermann Wagner and Alexander Supan have to be thanked for the prodigious labour involved in the compilation of this great census, the mass of figures in which is simply appalling. The number consists of 130 pp., and is crammed with information. The continents are taken in order, and, whenever possible, the census of the actual town is given in 1891. Details of the previous census are also given in each case, so that the reader may see at a glance the increase of population in a certain number of years. Towards the end comes a list of the large cities of the world and their inhabitants, and we extract the first six as having special interest:—

London (1891)	4,415,958
Paris (1891)	2,712,598
New York and Brooklyn (1890)	2,352,150
Berlin (1890)	1,763,543
Canton (1891)	1,600,000
Vienna (1890)	1,364,548

There are only 12 cities with over a million inhabitants, of which China rejoices in possessing four.

FRESH-WATER MEDUSÆ.

SPECIMENS of the fresh-water Medusa from Lake Tanganyika, the same probably as that referred to by Böhm in 1883, and Wissmann in 1887, have been received in good condition, and form the subject of a paper by Mr. R. T. Günther in the April number of the *Annals and Magazine of Nat. Hist.* The individuals vary in size from 1 to 1·8c. across the bell, while the largest measured as much as 2·2c. The umbrella is flattened; the tentacles very numerous, varying in length, and arranged to a sixth order with great regularity. Mr. Günther establishes a new genus, *Limnocnida*, for this interesting animal, and preserves Böhm's name of *tanganjicæ* for the species, that author having recognised the form as new, although he was unable to describe it in 1883 for want of the necessary literature. The fresh-water Medusa which thrives in certain years in the tank at the Botanic Gardens, Regent's Park, *Limnocodium sowerbii*, differs considerably from this new form.

THE PLAGUE OF VOLES.

WE have referred on several occasions to the subject of the plague of Field-Voles in Scotland, and we shall, therefore, merely chronicle the publication of the "Report of the Departmental Committee on Field-Voles (Scotland)," Parl. Paper—C.—6943, 1893, price

1s. 4d., which appeared early in the month. The Report traces the nature and origin of the plague; causes of the outbreak; effect on the pasture and on the stock; remedies; previous outbreaks, among which the Philistines and other biblical stories are quoted; natural enemies of the voles; conclusions and recommendations. Of course, the obvious remedy, and one which is hinted at in the Report, is the non-destruction of the birds and mammals that destroy the voles for food; but it is almost useless to insist on the preservation of the balance of life in Nature where the game-breeder and game-keeper are concerned. The owls, buzzards, and kestrels, the stoats, weasels, and foxes, are sacrificed to the game-god, and therefore the sacrificers bring other (and greater) evils on their own heads and on those of others. The Report is illustrated by figures of voles and their natural enemies.

A New Variety of the Pala.

In the last number of the *Proc. Zool. Soc.*, Mr. Thomas describes a new variety of antelope differing from the ordinary Pala by its much smaller horns and more slender skull recently obtained from Nyassa-land. The especial interest attaching to this form is that it occupies an "island" within the distributional area of the common Pala.

Costa Rica.

An excellent map of Costa Rica will be found in "Geografia de Costa Rica," by F. M. Barrantes (*Barcelona*, 1892). This work, which has been occasioned by the Exhibitions of Madrid and Chicago, gives a sketch of the territory, its limits and coast-lines, mountain and river systems, climate and productions. A list follows, of the birds, taken from Zeledon's papers, reptiles and fishes, and mammals. This last is a translation of Frantzins' paper, and occupies 72 pages. It is followed by a series of chapters on population, government, and religion, and then the book deals with the various provinces and islands. There are a great number of full-page illustrations, reproductions of photographs of town scenes chiefly, and one of the crater of the volcano Poas, which is most interesting, being taken from the edge, and showing the crater-lake. There is no index.

The Origin of the Dingo.

In a useful descriptive catalogue of the Mammals of Australia, recently issued by the Australian Museum, Sydney, Mr. J. D. Ogilby enters very fully into the disputed question of the origin of the Dingo, or native dog. After quoting the chief recent authorities who have written on the subject—more especially the late Professor McCoy—

the author observes that, in regard to the question whether the dingo "is an indigene or has been brought hither through human instrumentality, we consider, notwithstanding that the greater number of authors incline to the latter theory, that the recognition by Professor McCoy of fossil remains, in no wise differing from those of recent individuals, and contemporaneous with similar remains of *Thylacoles*, *Diprotodon*, &c., sets the question at rest, and goes far towards proving that the species is indigenous to continental Australia, and was an inhabitant thereof prior to its colonisation by man, no human remains of such antiquity having as yet been discovered." This conclusion is supported by the recent discovery of dingo remains in a stratum lying beneath 63 feet of volcanic sand, underlain by another 60 feet of blue and yellow clay, thus clearly indicating the great antiquity of the animal in question.

From our own experience of the specimens in the British Museum, we have hitherto believed that remains of the dingo only occurred in the Australian cavern-deposits, where, we believe, human remains also occur; but these new discoveries seem to indicate the existence of this dog at a period when it is almost impossible to conceive that man had reached Australia. Startling as it may seem, we are, therefore, inclined to believe that the dingo may really be an indigenous Australian mammal. There are, indeed, several indications that we shall ere long have to revise our views as to the origin of the mammalian fauna of Australia; and the number of placental land mammals indigenous to that country noticed in the catalogue before us shows how incorrect it is to speak of Australia as exclusively a land of Marsupials and Monotremes.

Admitting that Mr. Ogilby may very probably be right as to the indigenous origin of the dingo, we can, however, scarcely follow him when he states that "until proof to the contrary is forthcoming, we shall consider the honour of being the original progenitor of our household favourite as the due of the Australian warrigal" (another name for the dingo). Surely the author is aware that in Europe the remains of dogs closely allied to the wolf occur in Pliocene beds, and that there is every reason to believe that the Eskimo dog, as well as sheep-dogs, are more or less directly descended from the wolf, while the Hare Indian dog of North America is just as closely related to the coyoté. The origin of the numerous breeds of domestic dogs cannot, indeed, be disposed of in this off-hand manner.

NESTING OF THE GOBIES.

A PAPER, by F. Guitel, which has appeared in the last number of Professor de Lacaze-Duthiers's *Archives de Zoologie expérimentale*, contains interesting notes on the hitherto little-observed habits of one of our commonest coast-fishes: *Gobius minutus*. The Gobies belong to the few fishes which construct a nest for the shelter of

their eggs, a work which, as in our Sticklebacks, devolves on the male parent alone. Although the fact of Gobies making nests has been known since the time of Rondelet (1554), no sufficiently-detailed account of the breeding habits of any species of the genus had yet appeared. The male *Gobius minutus*, for the nest, in tidal pools chooses usually the shell of some bivalve, frequently the Cockle, under the concave surface of which the sand is hollowed out and cemented by a special mucilaginous secretion from the skin of the fish; a cylindrical tunnel gives access to the nest, which is covered over with loose sand. The female having deposited her eggs, fixed to the shell, the male watches over them, and courageously defends his nest during the whole period of incubation, lasting from six to nine days.

The Oligodynamics of Living Cells.

A posthumous paper of Carl von Nägeli has just been published at Zurich, with the mysterious title "Ueber oligodynamische Erscheinungen in lebenden Zellen."

Towards the end of the discussion, we read that living cells may succumb to three different forces. Of these the Physical and Chemical are by far the most frequent and fatal; a third, of comparatively rare occurrence, he chooses to term "Oligodynamic." The paper is occupied by an examination of this peculiar power.

The first startling announcement runs "fatal effect of nominally pure water on living cells." Nägeli was led to the conclusion that water possessed this property while testing the reduction of silver salts by living protoplasm. Solutions of the silver salts were made in the "nominally" pure water, either distilled or spring water, and the cells experimented on were those of Spirogyra.

In strong solutions, he noticed that the effect was chemical, the plants dying from poison; the cells lost their turgidity, the protoplasmic layer separated from the wall, and the chlorophyll bands changed colour.

With a solution so diluted that the merest infinitesimal trace of the salt only was present, the result was again death, but after a wholly different fashion; the protoplasmic layer still adhered to the wall, and the cells retained their turgidity, while the chlorophyll bands shrunk together into a round ball. The effect was sometimes very rapid, and in four or five minutes the cell was killed. This, then, is the oligodynamic phenomenon, and, as he discovered at the end of a long research, was caused by molecules of copper present in the water.

The "nominally" pure water was brought through leaden pipes ending in a brass tap. If the water was allowed to flow some time before being drawn for use, it was neutral; the experimenter could thus at will command oligodynamic or neutral water. In the case of

distilled water, the vessels employed in distillation were accountable for the fatal effects.

River, lake, or marsh waters were neutral, but were rendered oligodynamic by the immersion of coins, and again could be neutralised by the addition of some substance, such as sulphur, wool, cellulose, &c., which would expose a large surface, and so attract the copper molecules.

The paper is prefaced by Schwendener, who informs us that Nägeli intended to name the newly-discovered power Isagität, but later he changed it to Oligodynamic.

Professor Cramer, of Zurich, repeated the experiments, and in a most interesting paper gives us his results, which confirm those arrived at by Nägeli.

THERE is a brief and succinct account of the Sandgate disaster, by Mr. William Topley, in the April number of the *Geographical Journal*, which contains some curious references to earlier landslips in the same area, 1716, 1725, 1786. Mr. Topley delivered an interesting lecture on the subject to the Geologists' Association on April 7. A Report of this will be printed in the May number of the *Proceedings*.

IN the *American Geologist* for March, Charles Schuchert writes on the classification of the Brachiopoda. The genera (author and date quoted) are arranged in families, many of which are new, and there is one new genus, *Dallina* (Beecher), founded on *Waldheimia septigera*, Lovén. Following the classification, is a "Geological Distribution of the Brachiopoda," a most useful list, but which would have been of more service if arranged alphabetically, for the alphabet is stable, while systematists have their idiosyncrasies. Dr. C. E. Beecher has assisted Mr. Schuchert in his work.

WE have received, from the University Press of Chicago, the first number of a new "Semi-Quarterly Magazine of Geology and Related Sciences," called the *Journal of Geology*. It is excellently printed on hot-pressed paper, and has seven editors and thirteen associate editors. The first article is one on the Pre-Cambrian Rocks of the British Isles, contributed by Sir Archibald Geikie; most of the remaining space is devoted to questions relating to the glacial geology of America.

A PARAGRAPH has appeared in the daily Press reporting the discovery of coal in Essex. We understand that the report is without foundation, though it is by no means improbable that Coal-measures may occur at a moderate depth in that county.

THE *Mediterranean Naturalist* for March contains several interesting papers relating to the district around Malta. Surgeon-Captain M. L. Hughes continues his "Natural History of certain Fevers occurring in the Mediterranean"; Gaetano Platania publishes some "Geological Notes of Acireale," and Dr. Alf Caruana Gatto writes on "The Vegetation of the House Terraces of Malta."

IN the *Naturalist* for April Mr. John Cordeaux continues his "Bird-notes from the Humber District in the Winter of 1892–1893." The arctic severity of the winter led to a great rush of various species on the East Coast, all wild fowl, such as geese and duck, having been exceptionally abundant.

THE same journal has a curious note by Mr. H. Moody Foster on herrings confined in a brackish-water pond communicating with the Humber. The fish are dwarfed, and are easily captured by rod and line.

MR. E. T. NEWTON will deliver a lecture on the Reptiles of the Triassic Sandstones of Elgin before the Geologists' Association at their next meeting on May 5. The remains, or restorations, of these extraordinary animals will be shown by the oxy-hydrogen lantern.

MR. ROBERT T. HILL, in the *American Journal of Science* for April, gives a description of the Cretaceous formations of Mexico. The geology of Mexico is so imperfectly known that this contribution will be welcome. We observe that the vast mass of blue or grey limestone, known in Mexico as "Mountain Limestone," which forms so important a constituent of the Sierras, is now considered to be entirely of Cretaceous age. True Palæozoic limestones are almost entirely absent from Mexico.

IN the April *Scottish Geographical Magazine* will be found an interesting account of the Nile Valley, its physical geography and geology. The outline is particularly valuable, being written by Colonel Justin C. Ross, who, for several years, was Inspector-General of Irrigation in Egypt, and has had occasion thoroughly to study the subject of Nile deposits and floods. The want of a contoured map, unfortunately makes it somewhat difficult to follow the details as to the areas liable to floods, those in which salt tends to accumulate in the soil, etc. Nothing has recently been done to test the depth of the ancient deposits of the Nile, the boring to a depth of 345 feet at Zagazig, made in 1888, being the latest recorded.

MR. MALCOLM LAURIE deals in the current part (vol. xxxvii., pt. i.) of the Edinburgh Royal Society's *Transactions* with some new forms of Eurypterid remains from the Upper Silurian Rocks of the Pentland Hills. So rich was a particular seam of fine-grained sandstone in these extinct "Sea Scorpions," that no less than five new species, including a new genus (*Drepanopterus*), were found. An interesting feature of these species was the large size of their eyes, and the author hints at a possible deep-sea habitat. It is known that many deep-sea creatures, particularly among Crustaceans, are endowed with abnormally large eyes, "as if to make the utmost possible use of the failing light," which (according to one naturalist, at least) does reach even the deepest abysses of the ocean.

THE Transactions of the London Meeting of the International Congress of Hygiene and Demography, held in 1891, have just been issued in the shape of thirteen volumes. These include numerous papers on Bacteriology and other subjects of interest to students of Natural Science.

IN a letter to M. Daubrée, recently read to the Academy of Sciences, Baron Nordenskjöld has called attention to the wide distribution of certain metals hitherto regarded as rare. His analyses of the ash of an anthracitic substance occurring in large nodules in the old rocks near the iron mines of Norberg and Dannemora, in Sweden, showed the presence of the oxides of nickel and uranium (the latter to the extent of 3 per cent.), and the rare earths of cerite and gadolinite.

THE committee of the Alpine Club are taking steps, at the suggestion of one of their members, Captain Marshall Hall, to collect information from colonies and other countries as to glacier motion and history. Such of our readers as are about to visit glacial districts can much assist. Photographs are especially desirable, of shrinkage or increase, and of other changes, and evidence as to any part taken by ice-action in lake formation. They will find an article upon rough surveying and cognate subjects in the *Alpine Journal* for February, 1891, written by the above-named gentleman, who has consented to deal with the materials which it is hoped will result from the action of the committee.

THE Geologists' Association has planned to visit Bath at Whitsuntide. Excursions are arranged to see the Fuller's Earth works at Midford; the Forest Marble, Bradford Clay, and Great Oolite, at Bradford-on-Avon; and the Corallian Beds and Iron works at Westbury, in Wiltshire. It is also expected that one day will be devoted to the classic region of Dundry Hill.

I.

Natural Selection and Lamarckism.

MR. HERBERT SPENCER still believes in the Lamarckian factor in evolution as strongly as ever. In the pages of the *Contemporary Review* he claims such an "Inadequacy of Natural Selection" as necessitates the introduction of the inheritance of the effects of use and disuse as the only means of accounting for the facts he brings forward. I propose to examine his latest arguments, and to show that they are too weak to bear the superstructure which he erects upon them.

The first proof of use-inheritance[1] that Mr. Spencer advances is the fact of the striking differences in the acuteness of the sense of touch in various parts of the body as estimated by the varying distances at which the points of a pair of compasses can be distinguished as yielding separate and distinct sensations. He urges that these differences could not be brought about by Natural Selection, or survival of the fittest, and, therefore, must be due to the cumulative inheritance of the functional modifications produced by use and disuse in the individual. It is admitted, however, that a widely-diffused sense of touch would be evolved by Natural Selection, as being absolutely necessary for safety and continued existence; and it should be equally obvious that Natural Selection cannot act in an indiscriminately unvarying degree in all parts alike, but would evolve local sensibilities solely in proportion to the varying degrees of the usefulness which causes its action. Mr. Spencer himself admits that the high perceptive power possessed by the end of the forefinger "may" have arisen by survival of the fittest; and there ought to be no difficulty in extending the concession to the other finger-tips, and to other joints of the fingers and parts of the hand where it would prove useful, and where its partial diffusion would be aided by the general principle of correlation. The general distribution of the sense of touch is in accordance with the requirements of Natural Selection.

[1] I employ this term to signify "the inheritance of the effects of use and disuse." Besides being brief and convenient, it also has the advantage of not necessarily including the inheritance of such acquired characters as mutilations—a subject on which the more prudent Lamarckians appear to be rather sceptical. One of them, indeed, has distinctly denied the inheritance of mutilations and injuries.

Tactile sensibility scarcely exists in the internal organs, where it would be worse than useless. It is most delicate in the tip of the tongue, which keenly examines food by touch as well as by taste, and in the tips of the fingers, which serve as tactile and manipulative organs. The face, though including highly important organs, does not need so discriminative a perceptiveness as the tongue and finger, and accordingly possesses much less even in the lips and the tip of the nose, where its tactile sensibility is finest. The body and limbs, which have never needed a highly discriminative sense of touch, have never evolved such keen sensibility, so far as is known, and the Neo-Darwinian has, therefore, no need to adopt the suggestion of degeneracy under panmixia so superfluously made on his behalf. The back of the head and body has less perceptive power than the front, where it has been more needed, since we move in a forward direction, and meet or touch objects in front of us more often than objects in the rear. Great part of our tactile equipment dates back to very remote eras. Tongue, fingers, toes, lips, and nose must have evolved special tactile powers even before our ancestors had arrived at the Simian stage of evolution; and in passing through foliage, branches, and thorny bushes a fairly developed sense of touch in the face would be far more serviceable than in the back of the head, and the chest and limbs might well become somewhat more discriminative than the back.

While the relative sensibility of parts can in the main be accounted for by the Neo-Darwinian, it must be freely admitted that we cannot fully and decisively explain every detail, every minor modification, and every gradation, by the principle of the survival of the fittest. We may be unable, for instance, to explain why the thigh and fore-arm are less sensitive in the middle than near the end, or why the skin over the malar bone is less discriminative than other parts of the cheek, or why the tip of the nose retains a measure of tactile perceptiveness exceeding that of the palm of the hand; but our failure to unravel and clearly demonstrate every obscure cause of the minor facts of variation and evolution proves nothing. Even if we accept Mr. Spencer's view of the inadequacy of Neo-Darwinian factors, we still have to inquire whether use-inheritance will fill the gap, or whether it is equally or still more inadequate to afford the desired explanation. Does it help us to account for the fact that the tip of the nose is four-and-a-half times as discriminative as the back of the hand?[2] Is it reasonable to suppose that the nose comes into contact with objects far more frequently than the back of the hand does? Does it touch things just about as often as the third or lowest joints of the fingers, three times as often as the lower part of the

[2] The numbers in these comparisons might fairly be squared so as to represent the relative number of separate tactile areas in a given space instead of linear distances. Thus estimated, the tip of the nose is *twenty* times as sensitive as the back of the hand.

forehead, and nearly twice as often as the palm of the hand?[3] Is it probable that the palm of the hand touches objects so much less frequently than the immediately adjacent finger joints as to account for its possessing little more than half their perceptiveness? Is the cheek touched twice as often or twice as impressively as the forehead, and nearly three times as often as the back of the hand? Mr. Spencer's hypothesis does not seem to offer any assistance in explaining the facts. While it can be reconciled with them in some respects, it is in direct conflict with them in others. All it can do, indeed, is partly to explain such facts as are already explicable by the natural selection of useful tactile powers. Nothing is gained by the introduction of the Lamarckian factor, which, accordingly, still remains as unproven as before.

Mr. Spencer's explanation rests upon the probably correct opinion that tactile sensibility slightly increases with use, and upon the theory that acquired characters are transmitted to posterity. Now, the greatly increased tactile perceptiveness found in various parts evidently depends, as Mr. Spencer fully recognises, on an increased number of tactile areas possessing distinct and separate communications with corresponding sensation tracts or stations in the brain. Mr. Spencer urges, not merely that practice makes perfect, which would be a legitimate conclusion, but that it actually multiplies the lines of communication with distinct endings or areas in the skin and in the brain. Foster, however, whose authority in matters of physiology is at least as great as Mr. Spencer's, tells us that "The improvement by exercise of the sense of touch must be explained, not by an increased development of the terminal organs, not by a growth of new nerve-fibres in the skin, but by a more exact limitation of the sensational areas in the brain" (*Text-Book of Physiology*, pp. 532, 533, third edition). If Foster is right, how could *extreme* perceptiveness be evolved by the inheritance of a slightly-improved limitation or clearer definition of the partially confused and intermingled areas of sensation? How could the transmission of a more perfect working order explain the formation of large numbers of fresh nerve-fibres provided with fresh sensational organs in the brain, and ending in the skin in corresponding tactile areas which, in the end of the forefinger, must be some 900 times, and in the tip of the tongue some 3,600 times, as small and numerous as in an equal area in the back of the neck? It has always been supposed that increased use caused increased size. In this case it is conveniently, though inconsistently, supposed to produce the opposite effect. The more a tactile area is used the smaller it becomes, until these areas are as minute

[3] If we hold that the discriminative sensibility of the tip of the nose is inherited from ancestral lemurs, which habitually applied the nose to objects as an organ of touch as well as of smell, then the long-continued retention of such seldom-exercised perceptiveness shows that the inherited effects of disuse have been so exceedingly slow and slight as to be altogether problematical.

as in the tip of the tongue, where points only one twenty-fourth of an inch apart can be felt as separate impressions. But is it likely that mere use will diminish the size and multiply the number of the separate structures on which distinct sensations depend? Mr. Spencer leads us to infer that such multiplication had taken place in four cases which he tested[4]; but the assumption that increased use converted twelve tactile areas into fourteen or seventeen has somewhat of the same intrinsic improbability as the idea that a man by using his five fingers sufficiently might convert them into six or eight. Though processes of multiplication by splitting or subdivision are perhaps conceivable, Foster's explanation seems much more reasonable, and the supposed value of the whole case as an inferential proof of use-inheritance becomes correspondingly doubtful.

The next example or proof of use-inheritance to which we are referred is the decrease of the human jaw in civilised races. I have already dealt with this case elsewhere,[5] and have attributed the changes more especially to the combined effects of (1), panmixia; (2), sexual selection; and (3), the natural selection of an economy of structure which would promote lightness and agility besides saving some slight amount of nutriment. *Panmixia*, it seems, is a factor which Mr. Spencer had "excluded as impossible." But it is obvious that Natural Selection may favour large and efficient organs of mastication and strong unbreakable jaw-bones among low savages almost destitute of tools and cookery, living at times on the rudest and harshest food, and often falling victims to violence or accident. If Natural Selection has done this—which I presume will not be disputed—then it is perfectly adequate to cause *some* part of the difference between the jaws of savages and of long-civilised races; and it is only saying the same thing in a slightly changed form when we contend that panmixia, in the form of *non*-selection of large teeth and jaws, is one of the causes of the relative smallness of the teeth and jaw in civilised races. How far panmixia alone can reduce size is a matter not easily susceptible of clearly demonstrative proof, but it is at least obvious that the comparative cessation of selection of large strong teeth and jaws leaves the field more open than before for the action of reducing factors such as economy. If it still be said that Neo-Darwinian factors cannot reduce the jaw, we may point to the decisive fact that the other bones of the skull are distinctly lighter in Europeans than in Australians and negroes, although the skull as

[4] As no measurements were made *before* the increased use, the "clear proof" is clearly imperfect, for there is nothing to show that the sensitiveness of the finger-tips in the two blind youths and the two skilled compositors was not entirely congenital. Compositors capable of becoming "skilled," probably started with a sensitive tactile organisation. Weber's twelfth of an inch, too, is obviously only an approximate measurement, and ought not fairly to be made a standard of comparison for more delicate investigations.

[5] *Are the Effects of Use and Disuse Inherited?* ("Nature" Series.)

measured by its brain-capacity has greatly increased in size.[6] Mr. Spencer cannot possibly say that the thinness and lightness of the bones of the skull are due to disuse, or that the Italian skull has been shortened by disuse. Seeing that Neo-Darwinian factors can reduce skull-bones and soften the frowning glabella so conspicuous in Australian skulls, and that they can also absolutely or relatively shorten the whole skull, why cannot they reduce the jaw-bone and alter its shape and dimensions? Are we to suppose that a skull-bone that happens to be moveable is thereby exempted from the action of all evolutional factors except use-inheritance?

As panmixia and economy must be held to have reduced the weight of the mandible as of other skull-bones, so sexual selection must have modified its shape and size by repressing the brute-like muzzle and large prominent teeth seen in the lower and less humanised races. It has thus favoured the relative prominence of the chin, which is quite as marked a feature in the higher races as the reduction of the teeth and of the more purely dental portion of the jaw.[7] The effect of sexual selection—and of panmixia arising from the survival of delicately-constructed females under male protection and civilisation—is, I think, conclusively shown by the fact that the reduction in female European jaws, as compared with Australian female jaws, is about twice as great as in the case of men.

The reducing and disturbing influence of disuse *during lifetime* is also a further cause of decrease which cannot possibly be denied by the Lamarckian. When these various reducing factors are taken into account, what need is there, or indeed what room is there, for the introduction of the Lamarckian factor? The total reduction in the weight of the male European jaw as compared with that of Australian males, does not seem to be more than 17 per cent., and of this more than a third can be accounted for as being due to a much greater loss of teeth together with consequent alveolar absorption. Another third may fairly be attributed to the Neo-Darwinian factors which had

[6] Excluding the lower jaw, I found that the skulls of European females at the College of Surgeons (mostly Italian) were 12 per cent. lighter than those of Australian females, although the capacity of the cranium had increased by 9 per cent. The weight of male skulls was only reduced by nearly half as much.

[7] The reduction of the teeth appears to be greater than that of the jaw. Dr. Macalister found that the area of the crowns of teeth in Englishmen was over 16 per cent. less than in Australian males (*Nature*, Aug. 18, 1892, p. 380). This, in unworn teeth, would correspond with a reduction of about 23 per cent. in cubic measurement or weight—a much greater decrease than had occurred in the jaws that I weighed. A greater reduction of the teeth is exactly the opposite of what *should* have occurred if the Lamarckian explanation were the correct one—for as the teeth emerge from the gum already formed, and are but little, if at all, susceptible of alteration by subsequent use or disuse, they would either not be modified at all by use-inheritance, or at least would be modified much more slowly than the jaw—a circumstance recognised by Darwin, and often insisted on by Neo-Lamarckians, who say that the relatively rapid reduction of the jaw causes a frequent overcrowding of the teeth.

lightened the rest of the skull by over 5 per cent. in the fifty specimens (mainly Italian) which I weighed. Whatever estimate may be formed of the remaining portion of the reduction in the European jaw, we still have to deduct the effect of disuse during lifetime and the direct or transferred effect of sexual and social selection in repressing prognathism. What grounds can we have for assuming that any residue remains which can justly be claimed as a proof of the inheritance of the effects of ancestral disuse? [8]

In endeavouring to establish the inadequacy of Natural Selection, Mr. Spencer puts forth contentions or assertions which manifestly overpass the bounds of accurate and logical reasoning. He holds, for instance, that if Natural Selection does not cause "a *frequent* survival" of individuals possessing any particular trait, it can "neither cause nor aid" any change in that direction. But surely it is as rigidly inevitable as any arithmetical or mathematical fact that the *occasional* elimination of an unfavourable characteristic *must* proportionally affect the general average. Nay, the elimination of a single individual thus characterised has, *ipso facto*, altered the average among the survivors. There is not the least reason, therefore, for accepting the strangely unphilosophical dictum that an evolutional cause or factor will not act at all unless it acts in considerable or important degrees. We may safely allow science and reason to implant in us a perfectly firm and unwavering conviction that every cause, however weak, will produce its proportional effect; and we may expect that the long-continued and cumulative effect of even infinitesimal forces or tendencies may become perceptible in the course of sufficient time.

[8] Mr. Spencer has unintentionally exaggerated the extent of the diminution. A comparison of his statements (*Principles of Biology*, § 166, footnote) with the skulls which he inspected at the College of Surgeons will prove to any careful measurer that the jaw, which was too hastily assumed to be "an average English jaw," must actually have been the smallest English jaw in the collection—that, namely of a female skeleton (No. 70A) with exceptionally delicate features. Founded on such a basis, his ideas and statements concerning the "dwindling away" of the jaw go far beyond the sober facts. As Mr. Spencer's disciples thus learn to overrate the decrease of the jaw, so also they become apt to underrate the reducing causes. Thus Mr. F. H. Collins (compiler of "An Epitome of the Synthetic Philosophy," an abstract of Mr. Spencer's works and teachings, published with an approving preface from the hand of the master himself) represents the amount of nutriment required by the whole jaw as a grain a day and calls this "a large estimate" (*The Diminution of the Jaw in the Civilised Races an Effect of Disuse*, p. 12). A little arithmetic will show that the proportional share of daily nutriment for the jaws with their muscles, &c., is some sixty times greater than this, without including liquid. Master and pupil alike seriously underrate the extent of variation, and ignore the facts that the reduction in the jaw is largely correlated with a similar reduction in the skull as a whole, and that the reduction of weight to be moved would allow a proportional economy in the bones and muscles and limbs and body that carry the lightened head with its lightened jaw; and all this allows still further and complicatedly cumulative economy in the alimentary, circulatory, respiratory, and food-procuring organs, which only have to provide a lessened amount of duly prepared nutriment for the economised parts. Arithmetically estimated, the case for economy is made to appear hundreds of times weaker than a fair consideration of the relevant facts would justify.

Whatever may be said to the contrary, the "widely accepted" view that "Natural Selection will [tend to] increase *any* advantageous trait" is a sound one; for the very meaning of "advantageous" is that some advantage is conferred, that the welfare of the organism is aided, and thereby its survival and multiplication. Of course, the degree of power or influence of Natural Selection will strictly depend upon the degree of advantage, and in proportion as this is slight the corresponding effect will be slight, and may easily be counteracted or masked by the effects of other causes.

This idea that survival of the fittest can do nothing at all unless it is "frequent," is allied with further misconceptions of the way in which Natural Selection brings about evolutional changes. Mr. Spencer compares each generation with the preceding one and finds that the change is too slight to affect survival. I venture to insist, however, and with all possible emphasis, that the basis of comparison of advantage must *not* be the trivial amount of change occurring in each successive generation, but the much more notable variations seen in contemporary individuals. If individuals who are the most poorly adapted in any respect are occasionally cut off, while the more perfect are preserved, there may well ensue some amount of evolutional change, for the average may thus be altered, although the intermediate or mediocre majority were not appreciably affected by the eliminative and selective process. The differences or variations that appear are far greater than Mr. Spencer would allow us to suppose. Thus, some English jaws are two-and-a-half times as heavy as others, while he only concedes a possible variation of about 10 per cent. for Natural Selection to work upon, and makes this allowance, moreover, appear preposterously beyond the degree of variation which would actually be presented to the operation of selective factors. Such fanciful presentments of cases misrepresent the problem before us, and blind us to the actual methods of Nature, whose selective factors naturally act more decisively upon extremes than upon average mediocrity and the minuter degrees of variation. Those who think that Natural Selection acts only upon the almost imperceptible change in each generation, and that this change is too slight to be thus acted upon, ought logically to give up Natural Selection altogether. If the argument were valid, it is hard to see how any of our organs could have been evolved or influenced by Natural Selection. Even the evolution of our most rapidly-evolving organ, the brain, could not be thus brought about, for such evolution has been exceedingly slow, and the advance in each generation could be represented as much too insignificant to affect survival, besides being neutralised by the advantages of superiorities in other organs, as a Lamarckian might also urge.

Another curious limitation imposed on the potency of Natural Selection is its alleged inadequacy to bring about complex evolution. It is acknowledged that it can "keep all faculties up to the mark"

by destroying such individuals as have faculties in some respects below the mark. Natural Selection is thus credited with an all-round elevating power, for it raises all faculties to a higher level than they would otherwise have fallen to. Why, then, does its power abruptly cease at a certain line? If it can thus lift all faculties up to the mark under ordinary conditions, why cannot it raise them slightly above the mark if those conditions become more stringent? If it previously counteracted a general retreat all along the line, why cannot it now cause a general advance? Will an increase of eliminative and selective rigour produce no extra effect?[9] And if it raises aggregates of faculties, must it not raise the separate faculties of which the aggregates are composed? The complex evolution of many co-ordinated organs simultaneously is, of course, a much slower and more difficult process than the special evolution of a single organ or faculty which becomes exceptionally useful; but difficulty is not impossibility, and we are, therefore, fully at liberty to believe, with Darwin, that Natural Selection alone would suffice to bring about the many co-ordinated changes necessitated by the gradual development of the huge horns of the Irish elk or the long neck of the giraffe. As a matter of fact, complex evolution of special organs and complicated instincts *has* taken place without the help of use-inheritance, and in spite of the strongest opposition it can offer, as is seen in neuter insects, such as bees, and still more in some species of ants, where workers, soldiers, and another distinct caste, apparently of overseers, are descended from innumerable generations of helpless parents who do not develop various organs and instincts which the neuters alone can improve by exercise but cannot possibly transmit to posterity. To hold that Natural Selection is totally incapable of bringing about complex evolution is to hold that the evolution of bees and ants is impossible. Seeing, then, that Natural Selection has evolved the (relatively) huge head and fighting mandibles of the soldier-ant,[10] with all the complex co-ordinated changes involved, there is no reason for supposing that it cannot have effected similarly complicated changes in larger animals, such as the elk and the giraffe.

The influence of beneficial modifications in determining survival is not easily measured. Advantages may often be more decisive than we are apt to suppose. Thus the discriminative tactile sensibility of the tip of the tongue seems to have been of some importance in promoting welfare and survival. It greatly aids the sense of taste in securing fit and proper nutrition—a consideration entirely overlooked by Mr. Spencer. While constantly exploring small irregularities of

[9] This rigour might give itself fuller scope by first increasing fertility as a point of supreme importance or absolute necessity.

[10] Among termites, too, the soldier caste possesses "enormously large, hard, and strong heads, almost as big as the rest of their bodies, and these are armed with gigantic and very strong and sharp mandibles, while the heads and mandibles of the non-combatant workers are much smaller and weaker."

surface, it is minutely ascertaining the texture, so to speak, of the organic or inorganic substances submitted to the ordeal of its keen judgment, and by the general structure and softness of the particles or fibres it determines whether they shall be accepted as digestible food or avoided as injurious or innutritious. It detects and rejects sand, earth, grass, leaves, or other unpromising material with which men from mere laziness, or the pressure of famine, would otherwise gratify their hunger, to the injury of the whole system.[11] It discovers and enables us to remove such objects as fish-bones, hair, feathers, and pieces of grit (which latter, as I have personally experienced, may splinter a tooth) and so saves internal organs from injury by their accumulation or friction, and from waste of digestive activities. The tongue also delicately manipulates the food, and secures its proper mastication by pushing it between the teeth, while carefully avoiding being bitten between the little grinding mills and cutting machines amid which it works so deftly and yet so safely. It is the highly and wonderfully discriminative guardian and janitor of the whole alimentary canal, and the supreme judge of the quality, composition, and admissibility of the nutriment which is the foremost essential of life. If Natural Selection cannot evolve a very special degree of sensitiveness in the tongue, it is hard to see what it can evolve. Mr. Spencer, however, contends that "no advantage is gained" by the explorations of the tongue, that the extreme perceptiveness of its tip is not needed, that use-inheritance furnishes the only possible explanation, and that even if tactile sensibility is of use in detecting fish-bones and foreign bodies among the food, a degree of sensitiveness equal to that of the finger-tip (which is only half as sensitive as the tongue-tip) would have been sufficient. But who can fairly undertake to decide that the higher degree of sensitiveness in an important food-selecting organ would never promote the well-being of individuals so far as to lead to survival where others perished? If, moreover, lower powers occasionally led to elimination while surplus powers did not, there might easily be an evolution of surplus powers—a principle which will help to explain many of our keener and finer powers of eye and ear and brain, and the surplus powers of organs generally.

In all cases of minor advantages, especially those of economy and lightness, it must constantly be borne in mind that the survival test acts more specially and most strongly in occasional crises—such as those of illness, famine, war, accident, and other perils—through some of which all of us still have to pass from infancy onwards. In such cases, exceedingly minute advantages may sometimes turn the scales of life and death. Even now, although the rigour of Natural Selection is so greatly mitigated under civilisation, a slightly healthier, stronger, and better nourished body, due to lessened waste

[11] Some tribes, when hungry or starving, partly evade or deceive this tactile sensitiveness and repugnance by filling their empty stomachs with a fine clay, or a fine, flour-like powder composed of exceedingly minute shells.

of food or tissue, or to other slight advantages, may carry child or man through fevers otherwise fatal, or through physical dangers and hardships, or arduous labours and mental trials under which he would otherwise have succumbed. In the not very remote past, a lighter skull, by diminishing the weight to be moved, and so increasing a man's speed and agility, may have enabled him to save his life by flight from enemies, or to overtake and kill a less swift and agile foe. Considering correlated changes and advantages, and looking at the many and varied tests through which our species has had to pass, at the critical episodes of personal strife, the occasional narrow escapes from natural dangers, and the innumerable diseases, parasites, microbes, &c., that waylay us through life, and search out the weaker points, it becomes almost impossible to say that any advantage has been so minute as never to have affected survival. Under starvation, wounds, disasters, illnesses, men hover on the brink of the grave, and a hair's-breadth of advantage, a single grain of spare nutriment or lessened expenditure, may decide ultimate recovery, while any little disadvantage may prove the last straw that breaks the camel's back. All organisms pass through kindred dangers and diseases of which we know little and care less. As a rule, the lower animals suffer far more frequently and severely from starvation and enemies than man does; they multiply much faster, and they are eliminated with proportional frequency. In view of such facts, we are by no means bound to suppose that small economies can never influence life or death in their immediate or ultimate effects.

Darwin did not disdain to call in the effects of slight causes. He partly attributes the somewhat reduced size of our teeth to the principle of economy,[12] although a tooth is scarcely $\frac{1}{3000}$th part of the weight of a man's body. Mr. Spencer, however, thinks that we ought to feel certain that economy, panmixia, and reversed selection, all put together, cannot reduce the useless, exposed, and sensitive eyes of the proteus and other "blind fish and amphibia" living in dark caves. We are told, indeed, that "an economy of $\frac{1}{200}$th of the creature's weight could not appreciably affect survival" (p. 166).[13] But if Natural Selection were really incompetent to secure economy in small parts, it could not secure economy in the body as a whole.

[12] *Descent of Man*, p. 562.

[13] Such an economy is equivalent to the saving of three-quarters of a pound in man. Many circumstances are conceivable in which death might be avoided and multiplication be favoured by slightly increased facility and quickness of motion, by surplus nutriment, and lessened liability to various dangers. Disused parts retain, or, apparently even increase, their share of liability to tumours, ulcers, and other diseased conditions which occasionally prove fatal by their direct or indirect results—of which many examples are known in man, the only animal with whose pathology we are really well acquainted. Probably, too, as Ray Lankester points out, there was a preliminary selection of animals whose imperfect eyes led them into the sheltering darkness of the cave, and prevented them from straying from it.

If no saving can be effected in the small separate fibres that make up muscle and nerve—if economy cannot be brought about in the separate cubic inches, or ounces, or grammes, of the bodily structure, how can it possibly be effected in the whole body, made up of those separate inches, or ounces, or grammes? Mr. Spencer allows that Natural Selection, and, therefore, panmixia also, can act upon aggregates; and he is, therefore, logically bound to admit that they can affect the minute parts of which aggregates are composed. The changes are not necessarily as gradual as Mr. Spencer represents them. Greatly diminished, or almost aborted, eyes will occasionally appear, and the possessors, no longer eliminated through failure of vision, will diffuse the degenerative tendency among the species by intercrossing.

Fortunately, the question of the possibility of minute reduction and minute economy in the shape and weight of organs, without the aid of use-inheritance, can be settled by a simple and direct appeal to facts. The advice of Solomon, that we should go to the ant to learn wisdom, is peculiarly applicable to the Neo-Lamarckian. He can test his arguments by an appeal to Nature herself. In neuter insects we are furnished with a happy opportunity of excluding use-inheritance from the problem. Now, in neuter insects, the effects assumed to be impossible have actually occurred. Neuter ants of various species possess eyes in all manner of grades of imperfection and smallness down to the most rudimentary, and even to the complete loss of the eye, of which only a trace of the socket remains. This, like the loss of wings, cannot be due to the inheritance of acquired characters, since the neuters cannot transmit their acquired characters to posterity, and the actual parents still retain the eyes and wings necessary for the nuptial flight. What, then, is the use of elaborately arguing that accomplished facts are impossible? It is the ways and works of Nature that we must study. She cares nothing for the theories of philosophers and their fancied impossibilities. She calmly *does* the thing declared impossible; in neuter insects she reduces and economises on the minutest scale without use-inheritance; she neatly and fitly develops, with all due co-ordination and economy, the special organs of neuter insects, together with wonderful mental faculties, and complex social instincts, which are never exercised, or, indeed, possessed, by the parents. Why, then, should we be asked to believe that, without the help of use-inheritance, minute economy and complex evolution are incredible? And what argument can possibly show that processes which certainly take place in innumerable species of ants, bees, termites, and wasps cannot take place in larger organisms where we do not happen to possess any special means of demonstratively separating the effects of Neo-Darwinian factors from those attributed to the transmission of acquired characters?

With reference to the arguments advanced against Weismann, I may say briefly that, although the bodily, or somatic, elements which

shelter and nourish the reproductive elements can produce some kind of effect upon them, and though reproductive elements may persistently affect other reproductive elements, as in the case of Lord Morton's mare, yet such and similar facts do not in the least decide the question whether the cells, or units, of the bodily structure can affect the reproductive substance, or germ-plasm, in such a way as to convey their particular acquired modifications, or changes, to posterity.

A few words are also necessary in answer to the claim that since congenital characters are heritable, the *onus* of proving that acquired characters are "not inheritable" falls upon the Neo-Darwinian. The implication that the one case affects the other cannot be sustained. It is perfectly certain and perfectly well-known that there is a wide difference between the two classes of characters, and that they are not comparable in their degree of effect upon heredity—otherwise there would be no more dispute concerning the one class than concerning the other. No one questions the immediate and decisive heritability of *congenital* mutilations and malformations, or of any other congenital peculiarity, however striking or minute. On the other hand, it is a matter of general experience that the loss of a tooth, or eye, or limb, or finger-joint, is no more transmitted to offspring than a sunburnt complexion, or a clipped beard, or a knowledge of Latin or cookery. In proportion as the nature of each case admits of clear decisive proof, we see that acquired characters are *not* inherited; no indisputable evidence to the contrary has ever been given. The presumption arising from the *proven* facts, therefore, is that non-inheritance of acquired characters is the general rule, and the *onus probandi*, therefore, rests with those who assert the contrary. We cannot prove a universal negative; and it is not easy to find or suggest absolutely decisive tests of the admittedly slight power of use-inheritance, or of the size-reducing power of panmixia. The effect of use-inheritance on offspring is imperceptible. Darwin acknowledges that several generations must elapse before any appreciable result follows;[14] and during this interval other factors would be at work. We perceive the apparently insuperable difficulty, or practical impossibility, of excluding all other factors in such delicate and complicated cases. But if the Neo-Lamarckian will accept his own cases, and allow us to adopt his own assumption that extraneous factors are adequately excluded, we can show that the effects of use and disuse are *not* inherited in some features just as conclusively (or inconclusively) as he shows that, in other respects, they are inherited. If, for instance, use-inheritance is proved by the shortening of the less used wing-bones of the domestic duck, or the leg-bones of the rabbit, then it is disproved by the thickening of these bones; and, conversely, if it is proved by the marked thinness or lightness of the wing-bones of the

[14] *Variation of Animals and Plants under Domestication*, vol. ii., p. 288.

domestic fowl, it is also disproved by a non-shortening, or extremely slight shortening, which is most conspicuous in the breeds that never fly. But the Neo-Lamarckian will probably spike his own guns rather than allow us to use them against him; and we must allow him to do so, if he chooses. The one thing clearly provable is the excessive dubiousness of a factor which is so strangely uncertain in its action, or is so weak as to be easily overruled in some of its effects by causes so trivial and obscure that their interference was never suspected.

We have seen that the theory of use-inheritance is unnecessary, since Natural Selection and other non-Lamarckian factors are evidently capable of effecting such changes as have been adduced. When we further consider the many difficulties and improbabilities involved in the hypothesis—the suspicious lack of satisfactory evidence—the evidence to the contrary which can be drawn even from an examination of the details of the very cases presented as proofs—the ease with which the effects of Natural Selection and panmixia are attributed to use or disuse—the certainty that mutilations are not inherited even when (as in the case of the hymen) they have been repeated for innumerable generations—and the extreme difficulty of accounting for transmission of acquired characters except by the overwhelmingly cumbrous and improbable method of pangenesis—we may well come to the conclusion that the inheritance of acquired characters cannot safely be admitted as a factor of evolution unless much better evidence is produced than has hitherto been forthcoming; and until such proof of the Lamarckian factor is obtained, we can have no right to rely upon a mere hypothesis which biologists are fast rejecting as a mere figment of the human imagination. And if the growing opinion of scientific men is correct, Mr. Spencer's psychology and many cherished but illusive expectations of continued progress by education alone will prove to be but castles built on sand, and will have to yield due supremacy to selective ideals and methods corresponding with the fundamental laws of evolution and of mental and moral progress —a position which by no means excludes the beneficent effects of education and parental influence, although it rejects the delusive and mischievous belief in the alleged inheritance of the changes thus produced.

WM. PLATT BALL.

II.

Biological Theories.

IV.—SUPPOSED AUDITORY ORGANS.

THREE groups of phenomena appear to have been imperfectly distinguished in the minds of those who have ascribed auditory functions to the organs known as tentaculocysts in jelly-fishes, as otocysts in mollusca and some crustacea (*e.g.*, *Mysis*), as auditory sacs in lobsters, crayfishes, etc., and as auditory hairs or setæ in the same and other crustaceans.

The groups of phenomena are those associated with the transmission of sound-waves in air, of sound-waves in water, and of waves of other kinds in water. The nature of the confusion will be seen by a consideration of the following quotation from Sir John Lubbock's "Senses of Animals":—

> "Hensen took a *Mysis*, and fixed it in such a position that he could watch particular hairs with a microscope. He then sounded a scale: to most of the notes the hair remained entirely passive, but to some one it responded so violently and vibrated so rapidly as to become invisible." "*That these plumose hairs then really serve for hearing may be inferred . . . . . from the observed fact that they respond to sound-vibrations.*" "Hensen's observations have been repeated and verified by Helmholtz." (*Of. cit.*, pp. 93, 94).

The portions of the passage which I have omitted refer to the existence of nerves in relation with these hairs.

Two criticisms are at once suggested. In the first place, the inference is unsound. Because the human eyelashes "respond to sound-vibrations," it is not safe to infer that they "really serve for hearing," and it is not any more safe to infer from the "observed fact" that certain plumose hairs in Mysis "respond to sound-vibrations" that these serve for hearing.

In the second place, there appears to have been a misapprehension as to what constitutes a sound-wave in water.

If a sounding tuning-fork be pressed against the side of an aquarium, two kinds of waves are produced in the water, surface-waves and sound-waves. The surface-waves are visible to the naked eye and travel slowly across the surface, that is, sufficiently slowly to be followed by the eye. Each of these is a *movement* of the water,

partly up-and-down but partly in other directions. The details of this wave-movement will have to be considered in a future article, and it is only necessary here to recognise that the wave consists of an obvious movement; that its velocity is small enough to enable the progress of the wave to be watched; that it is transmitted only through a superficial layer of the water, the thickness of which varies with the intensity of the disturbance; and, finally, that its propagation depends upon gravitation and surface-tension.

The sound-waves agree in number with the surface-waves, and there the agreement practically ends. These waves are waves of compression rather than of movement; their rate of transmission is about a mile a second; their propagation depends entirely upon the elasticity of the water, that is, the property by virtue of which the water resists voluminal compression, and regains its original volume after compression; the movement of the water involved in the transmission of such waves, even when the sound is of explosive intensity, is far too minute to be seen; this minute movement is, moreover, strictly to and fro in the direction of propagation of the wave; the wave is transmitted not through a superficial layer only, but in all directions through the water.

If the aquarium be divided into two parts by a water-tight partition of wood, glass, iron, or other solid, the surface-waves will be stopped by this partition, while the sound-waves will pass unimpeded through it.

Sound-waves in air differ from those in water in the amount of movement of the particles transmitting the wave. Here, also, the wave is one of pressure rather than of movement, though movement ten thousand times as great as that in an aquatic sound-wave is involved.

Some idea of the minuteness of these movements may be obtained from the determination made by Lord Rayleigh and recorded in Ganot's "Physics." The note experimented upon was f^{iv} produced by an organ pipe. The intensity was such that the note was audible at a distance of more than half-a-mile. The movement of each air-particle was less than one two-hundred-and-fifty-millionth of an inch, *i.e.*, little more than one five-hundred-millionth of a wave-length.

Water is only one ten-thousandth part so compressible as air, and a like variation in pressure would, therefore, only involve in water a movement of each particle of about one five-billionth of a wave-length.

Whether Hensen's experiments were performed in a vessel sufficiently large for the accommodation of a series of waves each five billion times the length of the observed movements of the hairs or not, I cannot say,—the Atlantic Ocean would be a vessel of dimensions suitable for the experiment. What the notes were to which the hairs "responded" I do not know—suppose, however, the lowest

note which is audible to man to have been one of them—*i.e.*, a note produced by sixteen vibrations per second, or thereabouts. In water this note has a wave-length of half-a-furlong, *i.e.*, nearly four thousand inches. One five-billionth of this is one one-thousand-two-hundred-and-fifty-millionth of an inch ($\frac{1}{1250000000}$ inch). This can hardly be the length of the "violent" vibration which is said to have rendered the hairs invisible. Suppose the language used in describing the result to be not greatly exaggerated, we may safely say that a movement at least one million times greater than this was referred to. This is, however, the greatest movement producible in water by the passage of any audible note when the intensity is such as to render the note just audible at a distance of half-a-mile. The employment of notes of this low pitch, and of higher intensity than this, would be fraught with great danger, for the apparatus of the middle ear of man could hardly stand much more. To reach the intensity required for the production of the above result, a steam "devil" such as is used on large steamers would be required, and would have to be close to the ear of the observer who was watching the hairs under the microscope. The man who would risk putting his ear within a foot or two of such a source of sound when this was put into operation to the utmost limits of its powers would indeed be a bold man. I doubt, however, if the most powerful of these "whistles" ever made has ever been heard at a distance of thirty miles. Supposing $\frac{1}{1250}$ of an inch to be the extent of the violent movement rendering the hairs invisible, *i.e.*, a little over twice the diameter of a red blood-corpuscle, then the intensity of the sound required, supposing the note to be C_{IV} (=16 vibrations per second), would be such as to render it audible at a distance of 500 miles in air, and much further in water. Supposing it to be at such distance from the head of the observer as to have an intensity equal (in air) to that operating (in water) on the hairs, then the air in contact with his head would move with an amplitude of about eight inches, and with a force which no human skull could withstand for a moment. A microscope would be instantly destroyed by it. I doubt whether even any ordinary building could resist its destructive power, and do not for a moment believe it possible to construct an instrument capable of "sounding a scale" with such intensity as this.

We may, therefore, in spite of the authority of so eminent a physicist as Helmholtz, rest perfectly assured that the effects described have never been either produced or repeated or verified by either Hensen or anybody else. It is, however, not the record of the experimental results which is at fault, but the explanation of them. Vibrations in the water produced by the sound-waves in air, or by vibrations communicated from the source of sound through the table, microscope, etc., have been mistaken for sound-waves in water.

It is, perhaps, inaccurate to assume, as I have done, that the

intensity of a sound (whatever its pitch) is proportional to the range of pressure through which the transmitting medium passes during the transmission of the wave. Making allowance, however, for this error, the result would be the same. The intensity of wave, in water, necessary to produce a vibration of one-thousandth of an inch would in air produce a movement of about ten inches, and would involve changes of pressure which the human skull could certainly not resist, whether the walls of the building could or could not.

"Otocyst" is a name given to a sac, usually microscopic, lined with a sensory epithelium, usually, if not always, provided with sensory hairs. The sac encloses an "otolith," that is, a mass of some substance (usually calcareous) denser than the surrounding water or the tissues of the otocyst. The otolith may be replaced by several masses of some dense solid.

These, as the name "otocyst" signifies, are regarded, if not by all, at least by most zoologists, as auditory organs, and their action is said to depend upon the jolting of the hairs against the otolith or (which is worse!) *vice versâ*.

Perhaps what has been written above may convince zoologists that the production of any such jolting by an *aquatic* sound-wave is a physical impossibility. It is barely conceivable that any wave should involve pressures so great as to reduce the volume of any mass of substance to absolutely nothing, that is, to cause the matter of which it is composed to cease to exist. No pressure short of this would, however, bring the otolith into contact with the sensory hairs in question, even if they were, to begin with, at an infinitesimal distance. The fluid pressures we have to do with act equally in all directions. No movement, except in the direction of the wave-propagation, is produced: there can be no flowing outwards to the sides of even an infinitesimal amount of water which was between the tip of the hair and the otolith, for the pressure at the sides is equal to that between the hair-tip and the otolith.

Whatever the function may be of such organs as otocysts, tentaculocysts, auditory sacs, auditory hairs of marine crustacea, "ears" of fishes devoid of air-bladders, or, in fact, of any organ whatever, in any aquatic animal, that these organs, unless they be associated with a cavity containing a gas, should serve as auditory organs is simply a physical impossibility. An auditory organ would, moreover, be useless to such an animal as a jelly-fish, and the evolution of such organs in such animals could not be explained by Natural Selection. A real function of some of these organs will be treated of in a future article.

C. Herbert Hurst.

III.

The Fruit-Spike of Calamites:

A CHAPTER FROM THE HISTORY OF FOSSIL BOTANY.

FOR many years there has been much doubt and uncertainty among palæobotanists as to whether or not the fossil spike now known as *Calamostachys Binneyana*, Schimp., is entitled to be recognised as the fruit of *Calamites*. It was so regarded by Carruthers (1) in 1867 and by Schimper (2) in 1869, and Binney (3), who described it in 1868 as the fruit of *Calamodendron commune*, was practically of the same opinion, since the plant so named by him is one of the forms of *Calamites*. Williamson (4), on the other hand, has persistently maintained that its affinities are rather with the Lycopodiaceæ, and that the only spike entitled to rank as the fruit of *Calamites* is the one described by him in 1869, and more fully in 1888, and which is very different in many respects from *Calamostachys Binneyana*.

Recently the present writer has had an opportunity of studying a fine series of new preparations of the spike, belonging to Mr. W. Cash, of Halifax, and in a paper shortly to appear in the *Proceedings* of the Yorkshire Geological and Polytechnic Society, he claims to have fully established the fact that it is the spike of some form of *Calamites*. The time seems opportune, therefore, for a brief historical retrospect of the treatment accorded to *Calamostachys Binneyana* by those palæobotanists who have endeavoured to determine its affinities from a study of its structure.

As described by Carruthers in 1867, *Calamostachys Binneyana* consists of an axis, around which are placed in close array a number of whorled appendages. These appendages are of two kinds, whorls of sterile bracts alternating regularly with whorls of stalked structures carrying sporangia containing spores. The latter have peltate heads, and closely resemble the sporangiophores of *Equisetum*—so closely, indeed, that were the alternate whorls of sterile bracts suppressed or replaced by structures of the other type, the spike of *Calamostachys Binneyana* might well be taken for a spike of *Equisetum*.

The specimens at the service of Carruthers appear to have been imperfect and not well preserved, as the minute anatomy of the spike

is only partially given. Among other details, however, it is stated that the axis had "a bundle of fine scalariform tissue in its centre, forming about a third of its diameter, and generally appearing free from the surrounding cellular tissue, which is composed of somewhat elongated cells."

Throughout his description, Carruthers appears to have had no doubt whatever that he was dealing with the fruit of *Calamites*. In this he was probably guided by the great and undoubted similarity of the general characters of the spike to those of *Equisetum*, with which *Calamites* has always been held to have a close relationship. At any rate, he does not seem to have considered that the anatomy of the axis as he then understood it, *i.e.*, with scalariform tissue, and not parenchyma, in the centre, was an insuperable difficulty in the way of a determination supported by so many of the other characters of the spike.

Binney's description of *Calamostachys Binneyana* was published in 1868, and though his account is very unsatisfactory from the botanist's point of view, it implied the presence of a vascular bundle in the centre of the axis, as described by Carruthers. In spite of this, he accepted the view that the affinities of the spike were with *Calamites*, and does not seem to have considered it necessary to explain on this assumption the presence of the axile vascular strand.

In 1871 Williamson began that series of memoirs on "The Organisation of the Fossil Flora of the Coal-measures" which have done so much to increase our knowledge of Carboniferous plants. The first of these deals with *Calamites*, and incidentally the question of its relationship to *Calamostachys Binneyana* is referred to. It is pointed out (5) that in *Calamites* the centre of the stem is occupied by a purely cellular pith with a vascular zone at its periphery, while in *Calamostachys Binneyana* these conditions are reversed. Hence the inference is drawn that if the latter be the fruit-spike of the former, then in passing from one to the other the tissues must undergo a metamorphosis which is without parallel among living plants. In the fifth memoir (1874) the spike is described afresh, and many new details of structure are given with respect to it. Discussing its affinities in the light of the fuller knowledge, the author says (6):—

"After balancing these various facts and arguments, I am led to the conclusion that *Calamostachys Binneyana* has much closer affinities with *Asterophyllites* than with *Calamites*. With the latter it has no one feature in common. There is no solitary point in which the two plants resemble each other. The resemblance of the fertile sporangia of *Calamostachys* to those of *Equisetum* has been combined with the foregone conclusion that the *Calamites* were Equisetaceous plants, in leading to the belief that the two were parts of the same plant; but I cannot conceive of any conditions in which the stem of a Calamite could be prolonged into that of a *Calamostachys*. I have carefully investigated the relations which the fertile stems of the Equiseta bear to axes of their terminal fruit-spikes, and I find that their respective structures are typically identical. The transition from the stem to

the fruit-axis produces no structural changes save such as are of the most trivial kind; the general type remains unaltered and continuous. But to plant *Calamostachys Binneyana* upon the top of a Calamite would be as abnormal as to surmount the stem of an *Equisetum* with the strobilus of a Lycopod."

Thus, nearly twenty years ago, Williamson took up the position that, notwithstanding the close general similarity between this spike and that of *Equisetum*, the internal anatomy of the axis was so different from that of the stem of *Calamites* that its affinities could not be with the latter plant, but were rather with *Asterophyllites*. At that time he regarded the plant he described as *Asterophyllites* as a somewhat aberrant member of the Lycopodiaceæ (7), so that he virtually placed *Calamostachys Binneyana* in the same group.

In subsequent memoirs he has returned again and again to the affinities of the spike, but always with the object of strengthening the position taken up in the above extract. In the ninth memoir (1878) he expresses (8) surprise that Mr. Carruthers should still continue to believe in its Calamitean character, and points out that "both M. Grand' Eury and Dr. Dawson have fallen into the accidental error" of making him "regard the *Calamostachys Binneyana* as belonging to *Calamites*," whereas he has "most strongly opposed that idea." Two years later (1880) fresh specimens of the spike were described by him (9), with the result that they are said to have "fully confirmed the conclusions" arrived at in 1874, "that *Calamostachys Binneyana* had not the slightest relationship with the *Calamites*, but that it had strong affinities with *Asterophyllites* and *Sphenophyllum*." The following year (1881) is remarkable for the discovery of a new form of *Calamostachys*, which Williamson appears at first to have regarded as identical with *Calamostachys Binneyana*, but which he subsequently named *Cal. Casheana*, after its discoverer, Mr. W. Cash, of Halifax. After a detailed account of its structure, in which great stress is laid on the fact that it contains two kinds of spores, he says (10):—

"It is scarcely necessary to say that this discovery of macrospores and microspores in *Calamostachys Binneyana* supplies another link connecting this strobilus with the Lycopodiaceæ in the same measure that it separates the fruit from the Equisetaceæ. . . . This discovery strengthens my old conviction that the true affinities of this strobilus are with the Lycopodiaceæ."

Finally, in the fourteenth memoir, which appeared in 1888, we have his last pronouncement on the subject, which is as follows (11):—

"After a prolonged conflict the conclusions of those who have insisted upon the cryptogamic character alike of *Calamites* and *Calamodendron*, have met with an extensive, though not universal, acceptance. Meanwhile, both the opposing schools of palæontologists recognise the importance of discovering the fructification of these plants. Mr. Carruthers believed that he had found it in examples of *Calamostachys Binneyana*, and Mr. Binney arrived at a similar conclusion. I have always rejected these conclusions because of the conspicuous differences between the morphology of the Calamitean twig and that of

the axis of the *Calamostachys*. These differences appeared to me too great to make it possible for the one ever to have been a prolongation of the other."

Thus up to his latest utterances on the subject Williamson refused to recognise *Calamostachys Binneyana* as the fruit of *Calamites*, and justly insisted upon the difference between the anatomy of its axis and that of the stem of *Calamites* as being strong evidence against such a view. He even went further than this. So far back as 1874 he maintained (12) "that the only British strobilus of which the internal organisation has hitherto been described, that has any claims to be regarded as the fruit of *Calamites*," is that which he figured and described in the *Transactions* of the Literary and Philosophical Society of Manchester in 1869. Substantially the same statement is made in the memoir of 1888 already referred to, so that on both positive and negative grounds he refused to recognise any connection between *Calamostachys Binneyana* and *Calamites*.

Such was the state of opinion among palæobotanists when the new and much better-preserved specimens already referred to came into the writer's hands. Looked at from the botanist's point of view, it is obvious that the divergence of opinion is based upon the fact that there is a sort of inconsistency, if the expression may be allowed, between the external characters of the spike and the internal anatomy as hitherto understood. As already pointed out, the former have so close a resemblance to those of an *Equisetum* spike, that one naturally looks for the parent plant of *Calamostachys Binneyana* among those fossil forms most nearly allied to *Equisetum*, and by a short step we come to *Calamites*; but, on the other hand, the internal anatomy as described by Carruthers, Binney, and Williamson, is so irreconcilable with the anatomy of the stem of *Calamites* as to justify much that the last authority has written in opposing the Calamitean affinities of the spike.

An examination of the new material gave what the writer thinks is a complete solution of the difficulty thus presented, and at the same time indicated in what direction to look for the reconciliation of these diverse opinions.

In the first place, it soon became evident that the centre of the axis of *Calamostachys Binneyana* was not vascular, as had so frequently been stated, but was composed of cellular tissue which constituted a true pith. In 1891, when drawing up the first part of an Index to his Memoirs, Williamson introduced a brief footnote (13) in connection with *Calamostachys Binneyana*, which shows that he was beginning to have doubts on this point, and he admits that in the specimens described in 1874 "the elongated medullary cells" were "mistaken for tracheids." He says nothing, however, as to whether this parenchyma is a true pith, such as we find in *Calamites*, or merely a central parenchyma like that found in the stele or vascular cylinder of some *Lepidodendra*. On this point, however, the sections referred

to are very explicit, and leave no doubt that the former alternative is the true one.

In the next place, it became clear that the axis differed in a much more important point from the published descriptions of it, and that in a direction which had not been previously suspected. In fact, the specimens proved to demonstration that the cellular pith is surrounded by a ring of primary vascular bundles, which, in essentials, are of the true Calamitean type. The number of these is usually three, though there may be four or more, but, whatever be the number, each is characterised by the presence, at the apex of the xylem, of a carinal canal homologous with the canals found in the same position in the stems of *Calamites* and *Equisetum*, and which represent the tracheal initial strands of the primary vascular bundles. In the transverse sections, we find individual elements clinging to the sides of the canals, and projecting into their cavities, reminding us at once and forcibly of the appearances met with in *Equisetum*. So close is the resemblance, that one might from the transverse sections alone infer that these projecting elements are annular or spiral vessels, but for demonstrative proof recourse must be had to the longitudinal ones. Fortunately, some of these have been made in such favourable directions, and the tissues are so well preserved, that they leave nothing to be desired. They show us the carinal canals cut throughout their whole length, with the annular vessels still attached to the peripheral walls.

Hence it is obvious that the stele or central cylinder of the axis of *Calamostachys Binneyana* has a structure which is identical in essentials with that found in the stem of *Calamites*, and there is no such antagonism between the anatomy of the two as has hitherto been believed. Not only so, but when, as sometimes occurs, the formation of secondary xylem begins, it does so first at the carinal canals, as in *Calamites*, and subsequently in the intervening areas. In this way is brought about the triangular form of the pith, and the triquetrous arrangement of the vascular laminæ which surround it, and which Williamson compared with the secondary xylem of the plant he named *Asterophyllites*.

The conclusion, then, is, that the correspondence between the anatomy of the stem of *Calamites* and the axis of *Calamostachys Binneyana* is so close and definite as to leave no doubt whatever that the one is the fruit-spike of the other. Williamson's objections to this determination have all been based upon the supposed absence of such a correspondence, and now that it is shown to exist they naturally fall to the ground. The internal anatomy of the spike, then, points to the same affinities as the external characters, and together they supply all the evidence that is needed to establish the conclusion we have stated. To which of the forms of *Calamites* the spike is to be more particularly referred cannot at present be definitely stated, but there are some reasons for thinking that it must be sought

among those of the type of *Arthropitys*. This, however, is a matter of secondary importance compared with the fact that, after so many years of controversy and doubt, we have, at last, got clear and conclusive evidence that *Calamostachys Binneyana* is the fruit-spike of some form of *Calamites*.

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THOMAS HICK.

IV.

The Succession of Teeth in Mammals.

THE question as to the mode of origin of the successional teeth of Mammalia is one which has at various times received a considerable amount of attention from anatomists, and one which it may be of some interest to review in the light of recent researches by Dr. Kükenthal.[1]

In many of the Mammalia, as is well known, there are two sets of teeth, the first, or milk teeth, which are usually deciduous, and followed by a second more permanent set. In numerous reptiles and fish, on the other hand, we find a perpetual succession of teeth. Instead of the sets being limited to two, there is a constant replacement; as fast as one series of teeth wears away a new series appears to take its place. Moreover, in these reptiles and fish the replacement is also more complete than in Mammalia, for whereas in the latter we never find the whole series of teeth replaced, the true molars being never preceded or followed by others, in the fish and reptiles, when there is replacement of one there is replacement of all the teeth.

When the contrast between this diphyodont condition of Mammalia and the polyphyodont one of Reptilia is realised, the question most naturally arises, is the diphyodontism of the one a relic of the polyphyodontism of the other, or is it to be regarded as an advance from a monophyodont state, one in which a single set of teeth alone was possessed?

Two years ago much might have been said in favour of a primitive monophyodont condition by an appeal to the simplest Mammalia. In the first place, in the toothed whales, the Mammalia possessed of the most reptilian type of teeth and jaw, only one set of teeth was known. Again, in the Monotremes there is no replacement, and, indeed, the teeth which do exist are rarely used, being generally concealed beneath horny plates. Lastly, in the Marsupials only one tooth in each jaw, the posterior premolar, is ever replaced, and this seemed to give us the starting point for the more complete change occurring in higher forms.

Dr. Kükenthal's recent work has, however, invalidated two of

[1] *Ann. Mag. Nat. Hist.*, 1892.

these arguments, and in April of last year an authority as well qualified to speak on the subject as Mr. Oldfield Thomas, declared in favour of a diphyodont condition being the most primitive one in Mammalia—a condition, moreover, which was probably derived from the polyphyodontism of Reptilia.

The first argument drawn from the Odontocetes, the toothed whales, is shattered by the discovery of an embryonic series of teeth within those which later pierce the gum to persist as the teeth of the adult. In an advanced embryo of a dolphin (*Phocæna communis*), possessed of the twenty-five teeth in each mandible, rudiments of a second series were found internal to these. Kükenthal accordingly pronounces the teeth which are present throughout the life of these whales as representatives of the deciduous teeth of other Mammalia, while the uncut rudiments would correspond with the adult dentition of higher forms. These rudiments are, although small, perfectly recognisable as teeth, and are possessed of a cap of enamel and an inner core of pulp.

It was the Marsupials, however, that supplied the chief arguments to Sir W. H. Flower and Mr. Oldfield Thomas as to the secondary character of the diphyodont condition in Mammalia, and the most important part of Kükenthal's work on this subject is, therefore, the discovery of an almost complete series of successional teeth in some of the opossums.

In his classical paper of 1887, Mr. Oldfield Thomas,[2] after a considerable discussion of the teeth of Marsupials, arrived at the conclusion that originally all Mammalia were wholly monophyodont, the permanent teeth having ancestral priority over their ontogenetic predecessors of the deciduous series.

To account for the supposed secondary development of the deciduous dentition, it was assumed that a retardation of the permanent teeth would be beneficial by preventing overcrowding before the mouth was fully grown, while at the same time some provision of masticatory organs for a young animal with its teeth thus retarded would be essential. This advantageous retarding of the teeth, together with the necessity of finding some temporary grinding apparatus, was supposed to have resulted in the formation of a deciduous set of teeth which are of greater or less complexity, and retained for a longer or shorter time in different members of the Mammalia.

From the fact that only one tooth is replaced in the Marsupials, it was further supposed that the Mammalia had reached a fairly high degree of complexity before the necessity for deciduous teeth became urgent, since they could only have possessed one deciduous tooth when they had already gained all the complications of a generalised Metatherian condition. It was considered that at this juncture the

[2] *Philosophical Transactions* of the Royal Society, 1887.

Marsupials branched off from the main Mammalian stock and obviated their difficulties in other ways than by the development of a complete temporary dentition.

At the time of propounding this lucid and carefully elaborated theory, Mr. Oldfield Thomas pointed out that it would entirely collapse should a more complete replacement be discovered among any of the Marsupials, and he has been the first to acknowledge that this has now been done by Kükenthal.

Years ago Professor Huxley stated his belief that the deciduous dentition now found in Marsupials was but a relic of a fuller milk dentition, instancing an analogous loss in the case of the shrew, which has no deciduous teeth, while its near ally, the hedgehog, has a complete replacement. At the time, this belief seemed barely tenable, since in no Marsupial had any trace of a larger number of deciduous teeth been found, while even the one present in most was known to be absent in some forms (*Phascogale apicalis* and others). Moreover, fossil Marsupials, such as *Triconodon*, show the replacement of the posterior premolar, and of that tooth only. This, however, only affords one more instance of the danger of generalising from purely negative results, for Huxley's belief has now been amply justified by Kükenthal's discoveries, and diphyodontism is proved to have been the ancestral condition of at least the Didelphydæ.

It is, of course, possible that a complete cycle of development has been undergone, the milk dentition at first appearing as a provisional masticatory apparatus and afterwards persisting while their phylogenetic predecessors underwent abortion. There is, however, little or no foundation for such a suggestion; a far simpler explanation is to regard both series as representatives of two consecutive series of the Reptilian teeth.

In making this supposition, we must, moreover, grant some probability that the true molars were at one time subject to replacement, and this hypothesis also receives confirmation from the recent researches on the Marsupials.

Kükenthal appears to have been first led to work at the embryonic dentition of the Marsupials from the consideration of the synchronous development of the deciduous posterior premolar and the remaining permanent premolars. This suggested the idea that all the premolar teeth belonged to the same series, and Kükenthal has succeeded in proving that this is the case by the discovery of rudiments of an almost complete series of successional teeth.

These embryonic rudiments were found, not only for the incisors and canines, but also for the most anterior premolar, and for the first and second teeth of the true molar series. The only rudiments not found were, therefore, those of the second premolar and of the more posterior molar teeth. Further, these vestigial rudiments of all the successional teeth are identical in structure with the developing third premolar, which is afterwards cut, replacing its deciduous predecessor.

The only possible conclusion to be drawn is that in *Didelphys* (and probably in other Marsupials) the teeth of the permanent dentition, with the exception of the last premolar, represent the deciduous teeth of other forms, while rudiments of a second series are present in the embryo, but only cut the gum in the case of the third premolar; these rudiments must obviously be the remnants of a more complete replacement.

If the two series of teeth in the Mammalia are to be regarded as relics of the many series in the Reptilia, the Marsupials, inasmuch as they have almost lost one of these sets, have, in this respect at least, departed further from the common stock of Mammalia than have many of the true placental forms. There is, however, not unfrequently, considerable variation in the extent of replacement, even within the same groups of placental Mammals; for instance, among the Rodents, the Leporidæ have their incisors replaced, while the rat is entirely monophyodont.

A further point of interest lies in the fact that, whereas in the Marsupials it is the second series of teeth, or that which corresponds with the permanent series of most higher forms, which has become degenerate, in other groups it is the precursory ones that are suppressed. We see this suppression of the milk teeth in some of the Carnivora: in the dog, for example, the deciduous teeth are well developed, the bear has much smaller ones, while in the seal they are still more rudimentary, and absorbed before birth.

E. C. POLLARD.

V.

The Recapitulation Theory.

A REJOINDER.

IF the value of a theory is to be measured by the number and the extent of the papers written, and by the amount of work done under its inspiration, then the value of the Recapitulation Theory is indeed great: but there is always room for the doubt as to whether an equal amount of work would not have been done without that inspiration; whether, for instance, Hyatt, Buckman, Jackson, and Beecher would not have published researches as great and as brilliant as those referred to by Mr. Bather in his article on "The Recapitulation Theory in Palæontology" (p. 281), even if they had been inspired by something less fantastic than the theory in question.

In attacking this theory, however, I was fully conscious of the enormous amount of work done under its inspiration; and in attempting to destroy the theory, I was fully alive to the possible effect upon the work of the future. I look upon the loss of all the "recapitulation" literature with perfect equanimity—nay more, I look forward with hope for the time when men shall cease to occupy the pages of our scientific journals with controversies as to whether "the ancestor" of the vertebrates was an ascidian, polychæte, nemertine, leech, turbellarian, sea-anemone, sponge, spider, crustacean, Balanoglossus, echinoderm or other *recent* animal of the embryos of which the controversialists happen to have been cutting sections. I do not think that if the writings thus briefly catalogued had been written without the "inspiration" of the Recapitulation Theory, they would have been less valuable. The little I do know about the psychology of "dominant ideas" leads me, indeed, to believe that the investigations would have been carried out, not only more fully, but also with far less error of observation. Under the influence of a dominant idea, men see what does not exist, and are blind to what lies before them; and what is even worse, what little they do see of the things before them is seen distorted. Whether the dominant idea be a true idea or a false one, if dominant, it is almost certain to lead to profound error. When first the idea of evolution gained a hold over men's minds through the influence of Darwin's and Wallace's theory of Natural Selection, it suddenly developed into a *dominant* idea in the

minds of those who accepted it incautiously. It was not the theory which was wrong, but the mental attitude of those who accepted it. It was not accepted as a theory, but adopted as a creed. Fortunately, those who did so adopt it were comparatively few.

With the Recapitulation Theory it is otherwise. *All* who have accepted this, have adopted it *as a creed*. Few of them can even see that it is not an explanation of anything whatever. They *believe* that it explains the series of events constituting ontogeny. In reality, it is only a dogmatic assertion that this series of events is controlled by some mystical "dead hand of the past" in such way as to serve as a record of events long passed. If the dogmatic assertion as to the course of events constituting the ontogeny had proved to be true, then it would be difficult to imagine any more interesting problem than that involved in the *explanation* of so remarkable a phenomenon.

The truth of the assertion has been put to the test, not of direct comparison of the ontogeny with the phylogeny in a large number of cases, for that has hitherto proved impossible (*pace* Bather), but by comparison of the supposed "records" with each other, and by other indirect means. The mere fact that of all the conclusions drawn as to the origin of the vertebrates every one contradicts every other, is one disproof of the theory. The fact that von Baer's law is in exact accordance with observed facts in a large number of cases is another and a more definite disproof. The two views—von Baer's and the Recapitulation Theory—are irreconcilable. Von Baer's is applicable only to certain animals, but its applicability to these disproves "recapitulation" in these. These animals are such as pass through a considerable portion of their development as embryos, or fœtuses, before entering upon the struggle for existence amidst untoward circumstances. The remaining animals, that is, those which have to fight for a living from very early stages indeed, are, by virtue of that early struggle, exposed to the modifying influence of Natural Selection; and, hence, if any "record" had existed in the ontogeny of such species, it must of necessity be very soon obliterated by this action of Natural Selection. The obliteration need not be complete. Some traces of the ancestral ontogeny (*not* phylogeny) may remain, and, so far as these traces have been made out, they also are in conformity with von Baer's law, and, therefore, inconsistent with the theory of recapitulation.

It by no means follows that no ontogeny is such as to present even a startling resemblance to the phylogeny of the same species. Mr. Bather's too brief accounts of the truly remarkable correspondence between what is known to be the ontogeny and what is supposed to be the phylogeny of *Antedon*, will sufficiently impress my readers with this fact, and it is "greatly to be hoped" that he will shortly enrich the pages of NATURAL SCIENCE with a very much fuller account, both of all the stages of *Antedon* and of all its supposed ancestors, *with figures of every one of them*, that we may all more clearly

see what are "the plain facts of palæontology," to which it is suggested (p. 281) that somebody's eyes (? mine) are "persistently shut."

"It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the *causal relation* between the two." (I quote from my previous article, p. 198.) This appears to have been overlooked by Mr. Bather, and the whole of his article is consequently directed against a position which I have never held. I have stood perfectly still, and his explosive bullets have rushed past me with a violence suggestive of a shower of meteorites: his aim was excellent; not a single stray bullet touched me.

Mr. Bather admits that "von Baer's law is undoubtedly true" in cases of fraternal relationship, but holds that "there are many objections to supposing that it is equally true" in cases of filial relationship. I had quoted Darwin to show that in the case of the pigeons where the relationship between *C. livia* and the rest is of this kind (the only case I know of which has been tested) the law is fully applicable. May I therefore ask for, say, half-a-dozen of the best of the "many objections" referred to?

In defence of myself against what has now, unfortunately, reached the public eye, I feel bound to correct some of Mr. Bather's misapprehensions as to my position. To each reply I will prefix the number of the paragraph in his article where the misrepresentation occurs.

(3. Middle of p. 276.) The larval stem of *Antedon*, if it be, as I suggested, the modified equivalent of the stalk of the ancestral larva, need not be supposed to be anything other than what Mr. Bather describes, nor yet need any conclusions be drawn from it (as to the stem of its ancestors) which differ in the slightest from the conclusions he has drawn from palæontological work.

(3.) I have not ignored "acceleration of development." The antlers of stags were given as an example of it.

(5. Bottom of page.) Nothing I have said will bear the interpretation here put upon it. Each transient ontogenetic stage may be a modified equivalent of a corresponding ontogenetic stage in an ancestor, and may yet be either more complex or less complex, or may even be so completely changed that no resemblance is observable.

(5. P. 277.) I do not feel bound to suppose, and have never supposed, anything at all comparable with what is here suggested. I do unhesitatingly say that the ontogeny described in *Antedon* is not an epitome of the ancestral *history*. There is a truly remarkable parallelism between the ontogeny and the phylogeny as here set forth. It is not a history, inasmuch as the correspondence between the two series is not one which could safely be *assumed* to exist apart from palæontological evidence.

(7.) Let it be assumed that the evidence set forth by Darwin

proves conclusively that the domesticated breeds of pigeons are descended from a wild species in no way differing from the existing *C. livia*. Then variation (in my narrowed sense of the term) has so affected the adult structure, under the influence of long-continued artificial selection, as to produce barbs, runts, fantails, &c. Direct observation of late stages of development in each of these breeds has shown (Darwin) that these stages also have varied in the *same* direction, though to a smaller extent. In each of these breeds the newly-hatched nestling is *less* like the adult *C. livia* than is the newly-hatched nestling of the "wild parent species," *i.e.*, the *C. livia* of to-day. In order to produce a "record" of the descent from *C. livia*, *i.e.*, in order that the late stages of development should reproduce the *adult* characters of *C. livia*, those late characters would have had to vary in the direction of *greater* likeness to the adult *C. livia* during the same time as the adult characters were varying in the direction of *less* likeness. That is what I have called varying in "another" direction. I did not say "opposite" direction, for the simple reason that stags' antlers, &c., were in my mind at the time, and it was obvious that, in such exceptional cases, the two directions were nearly or quite parallel.

(8.) I have never either denied or hesitated to admit the occurrence of such phenomena as Mr. Bather records in the Ammonites; nor do I dispute the *facts* made known by Würtemberger, Waagen, Branco, Hyatt, Buckman, or anybody else. What I have denied I will deny afresh at the end of this article.

(9.) I find no fact whatever (not even an assertion!) in the table given on p. 279 which in any way runs counter to any view I have put forward.

(10.) I have paid full attention to the opinions of palæontologists so far as I have become aware of them, but I bow to no *opinion* based upon an argument which is inconclusive. The mere opinion of the whole zoological and embryological world appears to be opposed to mine. Would Mr. Bather wish me to bow to that opinion also?

(11.) What Mr. Bather calls my "main fallacy" is one into which I have never fallen! I have never believed that existing species are descended from other *existing* species, except under the influence of artificial selection (as in pigeons), or geographical or other isolation.

(11.) All seven parts of the quotation tell equally in my favour. Not one tells against me.

(12.) Whatever I may seem in Mr. Bather's eyes to suppose, I can assure him that I never supposed any reasonable person to hold the Recapitulation Theory in any form demanding such a "harlequinade" as he describes. It was not in any such form that I either described or attacked it.

(13.) The reason for introduction of *Culex*, etc., into my paper will be obvious to anyone who will read the paragraph which

mentions them. (P. 196.) It begins with the words "Another kind of variation is illustrated . . . . " By reading to the bottom of the same page it may be seen how I "made capital out of them."

(14.) "Another mistake" of mine (as Mr. Bather calls it) is like my "main fallacy"! Not only did I *not* refuse to admit more than one kind of variation, I actually insisted on the necessity of recognising *at least* two kinds. (P. 196.) I am fully aware that each "kind" includes many varieties.

(14.) The remainder of this paragraph is unintelligible to me. "The very same '*change in the constitution of successive generations* of a species leading to the production of a new species' *does take place in the life-history of a single generation.*" And again:—"All these well-known instances of variation (and I use the term precisely in Mr. Hurst's own sense) 'occur in a way utterly unlike the way in which it does actually occur,' *i.e.*, within Mr. Hurst's horizon." What can this mean?

(15.) By making myself acquainted with certain brilliant researches "inspired by the Recapitulation Theory" to which Mr. Bather directs my attention, I have no doubt whatever that I may learn much. That the brilliant researches should have been inspired by that theory will certainly *not* make me reconsider my condemnation. The whole science of chemistry has arisen out of brilliant researches inspired by the theory of the transmutability of the baser metals into gold. Shall we therefore accept that theory? Mr. Bather is hopelessly at fault in his assumption that my attack on the theory is due to my ignorance of the many brilliant discoveries which have been made under its inspiration.

The "amended" statement of the method of variation given on p. 277 errs in the precise way in which Mr. Bather says that my paper did. It refuses to admit more than one kind of variation! Moreover, it speaks of events being "*pushed back*" *in time* by other events which have not yet happened. This is, no doubt, figurative, but whether any such erroneous conception exists in Mr. Bather's mind or not, the acceptance of the statement in that form by recapitulationists would certainly produce in the minds of the "*profanum vulgus*" an impression that this was a case in which the effect is produced before the cause has arisen.

To prevent any further misrepresentation as to what I have denied and what I have not denied, I will conclude with a summary statement of my position:—

I do not deny that a rough parallelism exists in some cases between ontogeny and phylogeny. I do deny that the phylogeny can so control the ontogeny as to make the latter into a record of the former—even into an *imperfect* record of it.

I assert that those very characters of the adult which vary most in the adult are precisely the same as those which vary most in the late stages of development also.

I do not deny that there may be *exceptions* to this last statement.

I deny that any one of the theories as to the ancestry of the vertebrates based exclusively on embryological grounds is worth the paper it was written on.

I maintain that much otherwise good embryological work has lost much of its value through the bias produced in the minds of the writers by the dominancy of the idea of recapitulation.

Comparative anatomy gives some help: Palæontology may give even more: by their aid embryological facts may be explained: but the facts of palæontology can never be discovered by the study of embryology.

"Vestiges," and these only, can give any embryological clue to past history which could not be equally well made out from comparative anatomy. In this last I may be wrong.

I maintain that the applicability of von Baer's law to even a single case would be a definite and final disproof of the Recapitulation Theory even in its most reasonable forms.

Finally, I assert that the applicability of von Baer's law in a large number of cases is capable of being readily proved by anybody, by a direct appeal to Nature, *i.e.*, by the study of a few vertebrate embryos taken at random.

C. HERBERT HURST.

VI.

Climate and Floral Regions in Africa.

ONE of the strangest features of African botany is the extreme poverty of species within the tropics, accompanied by a wealth and superabundance of vegetation, *i.e.*, of individuals, which can be hardly realised without actual observation. On the other hand, outside the tropics, both the number of species is astonishing (sometimes there are more known from a given area than is the case anywhere else in the world), and again, the number of individual plants on a given area is exceedingly small as compared with the tropics, and never shows the same extraordinary luxuriance.

The reason for this difference may be partly appreciated by noticing some of the distinct climatic conditions which exist in temperate Africa; each different climate maintaining, of course, its own set of plants.

Travelling in imagination along North Africa, from Gibraltar to Egypt, we notice the following changes. First, in Morocco, Algiers, and part of Tunis (roughly speaking, on the Northern Mediterranean side of the Atlas chain and its continuations, which run out in the sea near Tunis) we find ourselves in a Mediterranean climate. It is a fairly dry country, but does not suffer from drought to any severe extent. On an Algerian hillside, one sees numerous wild olive trees, many bulbous plants, and small shrubby perennials, while in sheltered places many annuals occur, but there is an almost complete absence of thick sward and the dense growth which one notices, for instance, in England. There is nothing like the crowding and struggle for existence which goes on in temperate Europe; every plant seems, as a rule, able to spread out, and the tint of the landscape is that of the underlying soil, often red, with a sort of grey-green haze, due to the scanty vegetation; the number of species is, however, fairly large. An examination of the plants themselves shows that they are mainly Mediterranean forms with numerous endemic species or varieties, whose origin from European species or their ancestors is intelligible enough if we remember the years that have elapsed since Tunis was in free land communication with Sicily, and since the Straits of Gibraltar were formed.

In Tripoli and the Bay of Syrtis, we are, in the desert of Sahara, exposed to the most severe and protracted drought. The plants that exist are very isolated indeed, in most places a foot or more apart,

and there are no plants whatever over wide areas. In spring, a considerable number of small annuals start up after the rains, flower and fruit in an incredibly short time, and die down immediately the country dries again; the residents are stunted, very often thorny bushes, extremely small and only able to resist drought either by covering themselves with a woolly coat, or by hiding their transpiration pores in cunningly-concealed pits or grooves which are so many traps to prevent the egress of the precious water. Some are said to surround themselves with an atmosphere of their own by exudation of essential oil, &c., but this is theoretical. Many have no leaves, others only have leaves in the wet season, and the few that keep their leaves are succulents or have them most beautifully adapted to their requirements.

When we proceed further along the coast and reach the little limestone hillocks by the sea at Alexandria, we are again among a different set of plants. They are very near the Algerian forms, but not exactly the same; probably they came from Palestine and are part of the Syrian flora as distinguished from that of the desert. Nothing is more exquisite than the flora of these nummulitic limestone hills. There are masses of the crimson *Ranunculus asiaticus*, the delicate little white star-of-Bethlehem, many species of *Astragalus*, and generally a wealth of colour and a beauty of form which I have never seen elsewhere.

On entering the Nile delta there is again a sudden and distinct change. The deep, carefully-irrigated alluvium is covered with a dense mass of plants, that is, if they are allowed to grow, for the soil is very valuable, and these very special characteristics of soil and water have produced the endemic Egyptian plants, such as the *Trifolium alexandrinum*, or "berseem" clover, of which some seven crops can be raised in one year on the same ground. Travelling up the Nile, we find ourselves before Assouan out of the delta flora, and again, as at Tripoli, in the great desert whose plants in many cases extend from Beluchistan to the shore of the Atlantic between Morocco and Senegambia.

Thus, in North Africa there are four marked floras more different from one another than are the floras of England, and one might almost say, Tibet, and to these must be added that of the higher peaks of the Atlas Mountains, which is of an alpine character, that is to say, five different sets of plants in Extratropical North Africa.

In Extratropical South Africa, beginning at the North, we find along the coast an offshoot of the evergreen humid tropical forest which has crept down from the Zambesi, and has occupied Pondoland, the lower parts of Natal, and, in an extremely modified condition so far as species go, formed the Perie Forest of King William's Town, and the Knysna Forest of Cape Colony. All these species of the eastern littoral, finding themselves in temperate and not tropical conditions, have varied greatly, and being separated from each other,

the forests of lower Natal, the Perie, and the Knysna have each their own endemic forms. Each is, in fact, a climatic island, and varieties suited to very local conditions have free play.

Further inland the plateaux of Mashonaland, the Transvaal, and upper Natal are covered with grass, among which are perennials and bulbous plants, and in places isolated shrubs or small trees, chiefly acacias. It is believed that there must be a communication with Abyssinia by ridges or mountain summits, at least 5,000 ft. high, in the tropics, and that the plants have travelled down along these ridges and summits without experiencing any great climatic change on the way, but this is rather theoretical at present. At any rate, they have taken on different specific characters *en route* in most cases, and they are quite different from those of the coast below 2,000 ft. Following the plants of these grassy plains southwards, we can trace them, often in very modified forms, on isolated mountain summits (such as at Somerset East) as far as the top of Table Mountain at Cape Town; but within the Cape Colony these grassy mountain tops, which are usually flat and table-like, are also inhabited by a wealth of endemic Cape genera and species, which probably ascended the hills from the lower parts of the peninsula. Every isolated mountain top may be in this way a climatic island, with species of its own. Still these plateaux and tables are generally in a fairly moist climate, and under quite different conditions to either the arid deserts of the Karoo or the western parts of Cape Colony and the lower parts of the peninsula.

In the Karoo and Namaqualand similar conditions prevail to those of the Sahara, and we find there bulbous plants, succulents, thorny plants, and densely matted perennials, similar in structural type to those of the Sahara, but of utterly different origin. The Cruciferæ and Chenopods, which are predominant in the latter, are almost absent here, and their place is taken by Mesembryanthemums, Stapelias, Pelargoniums, and others, which are not found in the north, and make a very different flora. Here, again, we find the landscape the colour of the soil, and scarcely tinted by the vegetation.

As for the distinctive Cape flora (that is to say, the remainder after the Karoo-Namaqualand and mountain-flora has been subtracted), it is different to any of the others; the conditions are rather dry, and yet not desert ones; the rainfall occurs at a different season to that of the eastern slopes and the Transvaal, and the prevailing type of plant is again different. Most are shrubby little perennials like the heaths, and a very large number have heather-like leaves; there are many bulbs, and scarcely a single annual of any sort or kind. The landscape is not wholly free from the colour of the soil, but it is masked by the grey-green tints of these little shrubs, and sometimes it is bright and rich in colour, as when *Erica Plukenetii* is in bloom. It is never, however, so beautiful as, for instance, that of the top of Table Mountain in the right month, which simply beggars descrip-

tion, and the conditions vary so greatly that in this peninsula there are probably more species than exist anywhere in the world on a similar area.

This makes, therefore, in Southern Extratropical Africa, four distinct floras, all, probably, closely united genealogically, but all specifically and very often generically distinct.

Turning now to Tropical Africa, a glance at the map and the exercise of the imagination will show that the enormous valleys of the Congo and Niger, up to from 3,000 to 5,000 feet, have been excavated out of a continuous tableland, and are in free communication with each other by the littoral of the west coast. Speaking generally, the whole of this enormous region is one continuous, dense, evergreen forest, and similar humid conditions prevail throughout its whole area. It, therefore, forms another flora comparable with the others and probably not nearly so much richer as might be supposed. The higher lands above 3,000 feet at 10° N. lat., and above 5,000–7,000 feet at the Equator, have another flora of a more or less similar type to that of Abyssinia, and the truly alpine flora of the Cameroons, for instance, seems almost specifically the same. In this region one can never see the landscape, as one walks usually through hedges of trees and shrubs 30 to 70 feet in height, or when the forest has been cleared, through tall grass which gives a realistic idea of the presumable view of a rabbit in a field of corn.

Hence, to show the relative abundance of floral regions, we have in North Extratropical Africa the South European flora in Morocco and Algiers, the Sahara desert flora, the Syrian flora at Alexandria, and the Egyptian or Delta flora, while in Southern Extratropical Africa there are temperate forest floras in Natal and the Cape, a grassy plateaux flora in the Transvaal and Cape Colony, the Karoo desert flora, and that of the Cape proper; that is to say, eight different sets of climatic conditions. In Tropical Africa there are the evergreen forest and the plateaux flora, that is to say, only two, of which the second is practically the same as that of the Transvaal, or is, at any rate, so close as to be scarcely reckoned as distinct. If one takes in the alpine flora of the highest summits of the Atlas, Abyssinia and Kilima-njaro, we get a total of eleven different floras.

The manner in which these have so far been studied reveals the difficulties of systematic botany. Boissier's "Flora Orientalis" mixes up part of the Algerian, part of the Sahara and part of the Syrian and Egyptian floras. Oliver's "Flora of Tropical Africa" includes the tropical forest, the Abyssinian and Transvaal grassy plateaux, and the alpine plants in part, and is about half done. Harvey and Sonder's "Flora Capensis" mixes inextricably the grassy plains of the Transvaal, the Karoo, and the peninsula, and is not half finished. Engler's "Hochgebirgeflora," containing the high mountain plants, is the only attempt to work on natural lines, and is also pre-eminently distinguished from the last two by having reached a conclusion.

G. F. Scott Elliot.

VII.

The Moas of New Zealand.

IN a paper in vol. xxiv. of the *Transactions of the New Zealand Institute*, p. 93, Mr. F. W. Hutton has discussed at great length the classification of the Moas of New Zealand. "My work," he says, "is founded on the measurement of the leg-bones of individual birds belonging to sixteen different species, and from these I have inferred with considerable certainty the proportion of the leg-bones in the other species." As to the genera, he says, "the crania of the different Moas are quite sufficient to indicate the existence of several genera. . . . After reducing the species to order, I find that they fall into seven well-defined genera founded on the crania, but generally accompanied by characters derived from the pelvis, the sternum, the absence or presence of a scapulo-coracoid, and the robustness of the leg-bones" (*loc. cit.*, p. 100).

In his paper Mr. Hutton gives a table in which the *Ratios in the Genera* are obtained, *as regards the long bones*, by dividing the length by the girth; and *for the skull* by dividing "the breadth at the squamosals" by "the height at the basitemporal," called ratio of breadth to height, and "the length from the supra-occipitals to the nasals," by "the breadth at the squamosals," to obtain his ratio of length to breadth. On examining this table we find that the maximum ratio of length to girth of the metatarsus in *Dinornis* is 3·6, and the minimum 2·5; while the maximum and minimum ratios in *Tylopteryx*, *i.e.*, 3·3 and 2·7, *both* lie within the maximum and minimum assigned to *Dinornis*. The same is the case with their tibiæ and the femora, so that as regards their long bones, those placed in *Tylopteryx* could all have gone into the genus *Dinornis*, for there is no reason why they should be assigned rather to one skull than to another. If we take the ratios of the crania (assigned to a great extent, therefore, arbitrarily to these limb-bones), the maximum and minimum ratios of breadth to height and of length to breadth in *Dinornis* fall completely within the ratios of *Tylopteryx*, and those of the sternum and pelvis of *Tylopteryx* fall within the ratios of *Dinornis*.

The ratios of the maxima and minima of both the metatarsus and the tibia of *Palapteryx* (the next genus further removed than *Tylopteryx* from *Dinornis*), fall also within those of the latter genus, so that as regards the metatarsi and tibiæ of *Dinornis*, *Tylopteryx* and

Palapteryx, there is no apparent reason for separating them. Take the next genus, *Anomalopteryx*, the maximum ratios of both its tibiæ and femora fall between the maximum and minimum of the genus preceding it, while the maximum ratios of the metatarsus, tibia, and femur of *Cela*—the genus following in succession—fit between the maximum and minimum of those in *Anomalopteryx*, while the ratios obtained from the metatarsus, tibia, and femur, in the next genus still, *Mesopteryx*, fit also between the highest and lowest ratios in *Cela*. It is evident that these ratios are valueless for differentiating the various bones into their proper genera *by measurement* alone, as is done in this paper. Indeed, if Mr. Hutton has correctly assigned to the genus *Euryapterx* three types of skull, why may not many of the varieties or types of cranium of Moa which we know not have belonged to one genus, even species, just as the cranium of *Gallus* or *Columba* does? It is also only in a few cases known certainly to what crania the numerous separated beaks belong, most of them having been only *fitted* together.

Turning to the question of species, the metatarsus of *Dinornis maximus* is separated from that of *D. excelsus* by extreme difference of ·5 inch in length and 1·25 in girth, and by a mean difference in the two species of ·5 inch in length and of ·7 in girth; while associated together as *D. validus*, we have metatarsi varying as much as 1 inch in length and ·8 inch in girth. The difference between the minimum tibia of *D. maximus* and the minimum tibia of *D. excelsus*, is 1·5 inch in length and 1 inch in girth, while we have tibiæ differing by 1·5 inch in length and in girth of 1·2 inch assigned to the one species, *D. validus*. Again, the maximum metatarsus assigned to *D. giganteus* lies midway between the greatest and least metatarsus assigned to *D. validus*; the minimum tibia assigned to the former is intermediate between the largest and smallest of the latter; while the maximum femur of *D. giganteus* is identical with the minimum femur of *D. validus*. The greatest difference in the metatarsi between the largest *D. validus* and the smallest *D. giganteus* is only ·5 of an inch in length, and 1·8 in girth; in the tibiæ 2 inches in length and 1·2 in girth; in the femora 1 inch in length and 1·4 girth. If we compare *D. robustus* with *D. firmus*, we find,

	In *D. robustus*.		In *D. firmus*.		A mean difference of	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	17·2–15·7	6·7–5·2	17·0–15·75	5·6–4·75	·08	·78
Tibia	32·7–30·0	6 8–6·2	33·0–30·0	6·5–5·3	·35	·60
Femur	15·5–14·4	8·1–7·2	15·25–14·5	7·5–6·0	·07	·90

Take another instance, and compare *D. ingens* with *D. firmus*:—

	D. ingens.		*D. firmus*.		Difference.	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	15·25	4·75	15·75	4·75	·50	·00
Tibia	29·50	5·30	30·0	5·30	·50	·00
Femur	14·25	7·25	15·25	7·50	1·0	·25

Is there any real reason why these metatarsi, tibiæ, and femora

should not all be assigned to the same species? Mr. Hutton has in his paper placed bones that vary by only ·5 of an inch in length in *different* species, while in the case of *D. validus* above, we have metatarsi differing by 1·5 inch in length and 1·2 in girth united in the same species. Why also should metatarsi whose dimensions lie between

12·2–13·0 ins. in length and 4·0–4·75 in girth be described as *D. torosus*.

those between

13·0–14·25 ,, 4·25–4·9 ,, ,, *D. gracilis*.

and those of

12·0 ,, 5·0 ,, ,, *D. struthioides*.

and why should femora of

11·50 ,, 5·5 ,, ,, *D torosus*.

and those of

11·25 ,, 5·5 ,, ,, *D. struthioides*.

We find in this lengthy paper also the following:—

Metatarsi measuring 10·4 inches in length and 4·0 inches in girth = *dromioides*
and ,, ,, 10·6 ,, ,, 4·0 ,, = *plenus*
giving a difference of 0·2 in length and ·0 in girth.

Tibiæ measuring 21·0 inches in length, and 4·0 inches in girth = *dromioides*
and ,, ,, 21·5 ,, ,, 4·6 ,, = *plenus*
giving a difference of ·5 in length and ·6 in girth.

and Femora measuring 9·6 inches in length and 4·0 inches in girth = *dromioides*
,, ,, 10·0 ,, ,, 4·0 ,, *plenus*
Difference of ·4 in length and ·0 in girth.

How unsatisfactory this method of classification is, may be gathered from Mr. Hutton's observations under *Cela curta*, Owen, where we read: "I regret that I cannot maintain *D. oweni* (described by Von Haast) as a separate species. It is only a small individual of *C. curtus* . . . Sir J. V. Haast says that the femur is shorter and the metatarsus more slender in *D. curtus* than in *D. oweni*; but my measurements do not bear this out, and the supposed anatomical differences between them are only individual variations, which may be found in almost every species of Moa. If *oweni* is to be separated from *curtus*, then for the sake of uniformity most of the species should be split into two, for they show quite as wide a range of variation." Now of *D. oweni*, the *metatarsi* are ·6 of an inch in length by ·5 inch in girth, and the *tibiæ* 1·65 inch in length by ·35 inch in girth less than *C. curta*; but the metatarsi and tibiæ of *P. dromioides* and *P. plenus*, which differ by a much less fraction from each other, are placed under different species. Nevertheless, it has been found necessary to unite Haast's species, *D. oweni*, though separated by greater differences, to *Cela curta*. It is very difficult to comprehend on what grounds these unions and separations are based, and impossible to assign unknown bones to their proper genera and species solely by this method of differentiation.

As for the identification of *Anomalopteryx antiqua*, it is founded on the photographed pieces of a metatarsus which Mr. Hutton had no opportunity of examining, and on fragments of two tibiæ, which

were, as I was informed by the mine-driver who excavated them, obtained from tunnels far apart, and many feet further under the lava, at a different part of the quarry from where the metatarsal fragments were obtained. These fragments are quite insufficient to determine with anything like the accuracy employed throughout his paper by Mr. Hutton the length of the tibia, which is, notwithstanding, entered (without a mark of interrogation as if certainly ascertained) as 12 inches. The chances are almost infinite against the two pieces being parts of the same bone. In speaking of these two fragments of tibiæ (which I examined carefully in the matrix, but found too imperfect to dare to identify), I say that they "were, undoubtedly, portions . . . of one of the greater forms." I should have expressed myself more accurately if I had said they were "undoubtedly not one of the smaller forms." As regards the metatarsal fragments which Mr. Hutton has assigned to this species, apparently for the reason that they were found in the same quarry, I have carefully compared them with the collection in the British Museum, and they agree most closely in form, though larger in size, with those of *D. curtus*. They belonged, however, *most certainly* to a species which was *not* "smaller than any which lived subsequently in the south island," as Mr. Hutton affirms. It cannot, therefore, yet be argued from them that the earliest known Dinornithidæ were smaller than those that succeeded them.

The differentiation of Moa bones into genera and species solely by the differences in their length and girth, I hold to be quite unreliable and misleading. Their form and outlines, I venture to think, are the only characteristics by which they can be accurately separated from each other. Before leaving New Zealand, I had already commenced to protract on paper the prominent features of the principal groups by referring them to rectangular co-ordinates which were drawn through the corresponding points of all the bones. By this method, I found that their points of agreement and difference, quite apart from size, could be strikingly exhibited. Professor T. J. Parker has recently contributed to the Zoological Society in a paper to be published in its *Transactions*, a classification of the Dinornithidæ, solely based on the crania of these birds; and as he hopes, I believe, to supplement it by a paper on the remaining bones of the skeleton, I trust he may give the suggestion I have made here a trial. The method will, at all events, I think, be found to afford an invariable standard of reference in describing the different bones. His forthcoming paper has, it is satisfactory to know, been collated with Mr. Lydekker's classification in the British Museum Catalogue of *Fossil Birds;* while, on the other hand, it is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with

that of Mr. Lydekker's catalogue,—a volume which had already reached the Colony before the reading of his paper, in which the classification is based on the type material in the British Museum, instead of adding to the already almost hopelessly involved synonymy of the Dinornithidæ, as his paper before the New Zealand Institute unfortunately does.

As to the age of the oldest fossil Moa bones found in New Zealand, Mr. Hutton, I find, dissents from my opinion as to the geological horizon in which the remains were found, which he is, of course, entitled to do. In stating, however, that "this conclusion was not arrived at by a re-examination of the sections at Mount Horrible and the Pareora, Mr. Forbes merely went to a quarry near Timaru, and, with 'little doubt,' identified a 'rough red shingle,' which he did not even see *in situ*, with the gravels of the alluvial fans of the Canterbury Plains," Mr. Hutton has affirmed what is not only quite incorrect, but what could not possibly be within his knowledge. He could not know how often I examined the geological structure of the region, or with what care, or what I have seen or not seen *in situ*. The shingle below the clay (or so-called loess) underlying the lava-bed, by which the age of the section is determined, I practically *did* see *in situ* on my first visit, and having subsequently re-examined the district, I have assured myself of the correctness of my previous observations and opinion. I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age. Notwithstanding Mr. Hutton's doubt as to the correct identification of the avian remains in the same bed in association with those of the Moa, I can, without hesitation, re-affirm that the bone I determined as *Apteryx australis*, and figured in the *Transactions* of the New Zealand Institute (vol. xxiii., pl. xxxvi., p. 368), really belonged to that bird. The bone, of which the drawing (for which I am responsible) is unfortunately not so good as a better draughtsman might have made, was, as I there pointed out, somewhat distorted by heat, but otherwise its identity was unmistakable. I am glad to find that Mr. Hutton admits "if it had been correct that bones of a living species of Kiwi occurred with them [the Moa-bones], it would have been strong evidence in favour of the bed being younger than Miocene." His observation that the figure in the plate illustrating my paper more resembles the femur of *Aptornis* surprises me, for the bones in the two birds are very dissimilar in form and strikingly different in size. The bone figured by me is represented of the natural size, and even in so poor a figure, the delineation of its internal structure recalls at once the section of an *Apteryx* femur.

Mr. Hutton, in the same paper, discusses the question, How long

ago did the Moa became extinct? My exploration of the Sumner Cave proved conclusively that the Moa and the Maori were contemporaneous (*Trans. N. Z. I.*, vol. xxiii., p. 373). This cave had been shut up by an extensive landslip, but for how long a period it was impossible to discover any evidence. The carving on the implements left on its floor was unmistakably done by Maoris. Along with the remains of Moas and other birds round the last fire-place of the inhabitants, I found the shells of Moas' eggs, with the shell membrane still intact, showing that the shells had not been part of any utensil, and their presence in the cave at all seems to leave little doubt that they were brought in as eggs for the purposes of food.

Now it is held by some authorities on Maori history and tradition that, because the Moa is rarely mentioned in their poetry or proverbs, this knowledge "dates from . . . . almost prehistoric times, long before the beginning of the genealogical descent of the tribes, which, as we know, extended back for more than twenty-five generations" (Colenso, *T. N. Z. S.*, vol. xii., p. 63). On the other hand, the Maoris now or recently living, especially in the North Island, have numerous traditions about the Moa, its colour, feathers, food, manner of life, fighting, and about their mode of capturing it. Mr. Hutton says, "So far as the North Island is concerned, I am compelled to believe that the Moas were exterminated many [400–500] years ago, because I feel sure, if it were not so, we should find as many allusions to it in Maori tales and poetry as we do to all the other birds, beasts, and fishes that were of interest to the natives." The date of the extinction of the Moa, I feel, however, cannot be reliably determined by reference to the native traditions or proverbs, for in Monck's Cave at Sumner, associated with the remains of the Moa, I discovered, for the first time, evidence of the former existence in New Zealand of a species of swan, somewhat exceeding the Black Swan of Australia (*Chenopis atrata*) in size.

Shortly afterwards, I received from various correspondents (chiefly from Mr. Hamilton, now of the Otago University), and from all parts of both islands, abundance of evidence that this bird had been at one time widely distributed, and had been used in great numbers by the Maoris for food down to comparatively recent times, for their remains were found in middens, precluding the possibility of their being of vast antiquity. In the Chatham Islands, also, I found, in abundance, bones of what I take to be the same species of swan, in ancient kitchen middens of the Morioris, in association with the remains of *Aphanapteryx*, and from localities there, as well as from the state of their preservation, which also precludes the idea of any great age. Yet though so abundantly used as food by these peoples, and especially by the Maoris, who have handed down, with great fidelity and exactitude, the names of most of their food animals, with their mode of capture, their traditions are entirely silent about this conspicuous bird, and, till two or

three years ago, no Maori scholar had even a suspicion that the swan was known to the natives before its quite recent re-introduction by Europeans, and if this omission from their traditions has taken place with one important bird, why not with another? In this case, the negative evidence relied on by Mr. Hutton is far from being convincing.

The enormous number of bones found by the early settlers scattered over the surface of the ground on both islands, which have now nearly disappeared, is evidence which seems to me to prove that the Moa existed down to quite recent times. This disappearance of the bones is attributed by Mr. Hutton to "the constant burning of the scrub by *Europeans* [for he evidently thinks (p. 151) that there were no fires before there were "human inhabitants to light the fires"], in which case the surface bones do not prove the late existence of the Moa in the districts where they were found." In the time (according to Mr. Hutton 400–500 years) since the Moa disappeared, there must have been all over the country fires ignited hundreds of times by lightning, and by stones rolling down hill-sides striking sparks from other stones and setting the parched grass, which is as inflammable as tinder after every nor'wester, on fire, which would travel farthest and burn most furiously just in those dry regions of Otago, where, Mr. Hutton believes, "bones, skin and ligament, once dried, and protected from the sun might easily be preserved for centuries." Then, also, the numerous little heaps of gizzard-stones lying on the surface of the ground (evidently voided by the birds), can, surely, scarcely have remained so little disturbed, even where protected by vegetation, for four or five centuries, in the midst of the denudation that goes on in New Zealand, by rain, frost and winds; for the gales that blow from the north-west in the summer can carry pebbles, as I have myself seen, larger than most of the Moas' gizzard-stones. It is very singular that Polack (whose book was published in 1838) should have been told by the natives that large birds (which he considered to be struthious) were then still living in the South Island, and this, it must be remembered, was before there was any suspicion of the existence of the Moa, and before the first bones had been discovered; it is not improbable, therefore, that the Moa may have been living on the South Island down even to the time that Captain Cook visited New Zealand.

Much, it is evident, still remains to be done before it can be said that the life-history of the Moas has been fully elucidated or their classification satisfactorily established.

HENRY O. FORBES.

SOME NEW BOOKS.

TYPES OF ANIMAL LIFE. By St. George Mivart. Small 8vo. Pp. viii. and 374. Illustrated. London: Osgood, McIlvaine & Co., 1893. Price 6s.

AUTHORS sometimes fail to do themselves justice by not selecting a sufficiently comprehensive title for their productions, but this omission certainly cannot be charged against Professor Mivart in regard to the present work. When we first opened the volume before us we not unnaturally expected to find a work somewhat on the lines of the late Professor Rolleston's well-known "Forms of Animal Life," in which typical representatives of the leading groups of animals of all kinds would be treated in the author's well-known style. Our surprise was accordingly great when we found that "Types of Animal Life" really meant, at the most, "Types of Vertebrate Life," and chiefly "Types of Mammalian Life." As a matter of fact, the work is exclusively restricted to those groups of Vertebrates possessing limbs differentiated into the full number of segments, and consists mainly of essays on the leading groups of Mammals; certain chapters being devoted to Birds, Reptiles, and Amphibians—apparently with the intention of pointing out the essential differences of the members of these groups from Mammals. As a whole, the work may be regarded as a companion to the author's recently-published "Elements of Ornithology," with a chapter on Birds which, in our opinion, might have been advantageously omitted.

The author commences his subject with a well-written dissertation on monkeys, as the representatives of the highest order of Mammals; and in the next chapter takes the opossum as his text for a sermon on the lower members of the same class. The next three chapters are devoted to the representatives of the lower Vertebrates coming within the scope of the work, namely, Birds, Reptiles, and Amphibians; while in the sixth chapter the author reverts to Mammals, taking the Carolina bat as a typical example of the Chiroptera. Then follows a chapter headed the American bison, in which the Ungulates are dealt with; while another, under the title of the racoon, introduces us to the Carnivores; and a third, designated the sloth, treats of the Edentates. In the tenth chapter, the title sea-lion appropriately designates a dissertation on the seals in general; while of the two remaining chapters, one is devoted to whales and sirenians (called by the author "mermaids") and the second treats of such Mammals as are not mentioned in the foregoing sections. The last chapter, by the way, is entitled "the other beasts," which leads us to remark that, in our opinion, it is decidedly a pity the author has thought fit throughout the work to follow the lead of the late Professor Parker in employing the objectionable term "beasts" in place of the now familiar mammals.

The work is fully illustrated and pleasantly written, and is admirably adapted for such readers as wish to gain a general idea of

the leading types of mammalian life, and their relations to birds, reptiles, and amphibians, without being encumbered with descriptions or systematic arrangement. The author has, moreover, exercised a wise discretion in avoiding, so far as possible, the use of technical names, which renders the book far more pleasant reading than several popular works we could name, and it is almost needless to mention that he is in general a master of style. We venture, however, to suggest that the less frequent use of the word "nevertheless" would save some weariness to the reader, and that when it is used the punctuation ought always to be the same (see pp. 150, 151).

The mention of punctuation reminds us that Professor Mivart stands in sad need of a proof-reader, not only in the present, but in some of his other works. For instance, on page 60 we are informed that the marsupial mole hails from America, while on page 182 we are told that tame gayals exist in certain parts of Spain! Again, on page 349 Soumerat stands for Sonnerat, while, if we mistake not, on page 292 Seaman does duty for Scammon. Moreover, it would have been well if the author had made up his mind on what plan he intended to transliterate Greek names, and we should not then have had *Chirotes* in one place (p. 146) and *Cheiromeles* in another (p. 165).

There are, however, other and more serious errors in the book which cannot be laid to the charge of the long-suffering printer, and for some of which the author will find it difficult to plead any adequate excuse.

One of the most glaring of these blunders occurs on p. 3, where the author states that the American monkeys, as compared with their Old World allies, "have developed an additional wisdom-tooth." And again, on page 32 we read that "the monkeys hitherto noticed, whether they have two or three wisdom-teeth on either side of each jaw, all agree with us in having three grinders, which have milk predecessors, and are technically known as premolars. But the marmosets alone have only two such on either side of either jaw." Now, in the first place, it is totally inadmissible to speak of all the molars as "wisdom-teeth," that name applying solely to the last of the series. Next, with the exception of the marmosets, all monkeys and apes, like ourselves, have three molars (wisdom-teeth); while those of the Old World have two premolars. The New World monkeys differ in developing a third premolar; while the marmosets are distinguished from these by the loss of the last molar, a true "wisdom-tooth," and thus have three premolars and two molars, and not, as the author states, two of the former and three of the latter teeth. Such a hopeless muddle as Professor Mivart has made of a simple subject would be hard to equal; and it is doubly to be deplored in a work intended to instruct the weaker brethren. It is, indeed, instruction (!).

We have already mentioned two slips in regard to distribution which may probably be attributed to careless proof-reading; but what are we to say with regard to the statement on page 184 that "the goats are exclusively confined to Southern Europe and Northern and Central Asia"? Has the author never heard of *Capra siniatica* of Egypt and the Sinaitic Peninsula, of *C. walie* of Abyssinia, or of the Nilgiri wild goat? Then, again, the statement on page 320, that the common rorqual is the largest mammal, is manifestly incorrect, the creature which has the honour of occupying this position being the blue whale (*Balnæoptera sibbaldi*).

Other statements show want of reading on the part of the author.

For instance, we find it mentioned on page 9 that the apes obtained during Hanno's "Periplus" were probably gorillas. Now it has been conclusively shown that the place where Hanno touched was Sherboro Island, in north latitude 7° 30″, or 4° 60″ north of the range of the gorilla! If the creatures then obtained were anthropoids at all, it is, therefore, evident that they must have been chimpanzees. So far, however, as we are aware, no one has disproved the conclusion reached by Mr. Winwood Reade, that the so-called "gorillas" were really dog-faced baboons.

Similarly we notice on page 352 a repetition of the old legend as to the harmonious relations existing between the prairie-dogs and the burrowing owls (*Speotito*), whereas Dr. Elliot Coues, and after him Captain Bendire, have expressly stated that the owls certainly prey on the young, and probably sometimes on the adults, of the "dogs." We believe, moreover, that the idea of flying frogs (p. 103) has been proved to be a myth. Then, again, we are told that the European beaver never makes dams, although it did so in the 13th century. Has the author, we may ask, any reason to discredit Meyernick's account of the dam-building beavers near Magdeburg in 1829?

In another passage (p. 116) the author observes that when "we descend to the Lias and Trias and Carboniferous rocks we come upon a great variety of" Labyrinthodonts. Now, although one of these creatures occurs in the Lower Jurassic of Russia, we are unacquainted with any Liassic representatives of the order, so that it is, in any case, incorrect to speak of Labyrinthodonts as occurring "in great variety" when we reach that formation. Later on (p. 373) the author does injustice to himself in stating that chevrotains—without any reference to the fact of the generic distinction of the African and Asiatic forms—are common to the Ethiopian and Indian regions; while, when giving hunting-leopards as distinctive of the latter region alone, he is incorrect.

Most of the illustrations appear to have been specially executed for the work, but we regret that we cannot congratulate the artist on the result of his efforts, none of the figures being good from an artistic point of view, while some—especially those of the seals—are positively bad. It would, moreover, have been better had the artist paid a little more attention to the relative sizes of the animals he depicts. For instance, it is decidedly misleading to find the figure of "the smallest anteater," on p. 256, far larger than that of "the great anteater," two pages back; and a similar remark will apply to the proportions of the koala, on p. 49, compared with those of the thylacine, on p. 47.

While commending the book as a readable and instructive one to those desirous of gaining some knowledge of vertebrate zoology, apart from technical details, we cannot but regret the presence of the errors alluded to above, which, we fear, will be accepted as gospel when coming from the pen of a zoologist of the high standing of Professor Mivart. R. L.

A History and Description of the Modern Dogs (Sporting Division) of Great Britain and Ireland. By R. B. Lee. 8vo. Pp. xiv. and 584. Illustrated. London: Horace Cox, 1893.

Till the appearance of Darwin's classic works on this subject, the study of the various groups of domestic animals, and the extraordinary modifications which have been produced in them by careful breeding,

were far too much neglected by zoologists; but at the present day these narrow views have been, to a great xtent, abrogated, although there are still some naturalists who affect to despise the subject. To those, however, who, like ourselves, are of opinion that a full a cquaintance with domestic animals is essential to a right understanding of evolution, the appearance of the present volume will be welcome as showing to what extent our modern dogs are still being modified under the selective care of the breeder. The work—which treats only of the breeds more or less closely connected with sport—is, of course, written for the breeder and sportsman rather than the naturalist, but the latter will be able to select the portions of special interest to himself and skip the remainder.

An especial feature of the book is the excellence of the illustrations, which are in the form of plates by one of the photo-engraving processes, and are far superior to those in any other English work of similar size. As a rule, no particular dog has had his portrait taken for the group he represents, but a "generalised" dog is in most cases depicted, and in this the author has been decidedly well-advised.

Mr. Lee's remarks on the scenting power of the bloodhound—in the course of which various old stories are disposed of—will be found of special interest, while as an instance of remarkable modifications produced of late years, the reader may refer to the account of the modern English breed of the dachshund. Following the practice of English shows, the author considers the great Dane as inseparable from the German boarhound; but if he will turn to the dogs depicted under these names in the third edition of Brehm's "Thierleben," he will find a considerable difference between them; and this is surely a subject in which the views of foreigners ought to receive a large amount of weight. We are glad to see that the author endorses the view that one of the so-called Irish wolfhounds was a boarhound.

The work ought long to remain the authority on the subject of British sporting dogs, and we hope that it may ere long be followed by a companion volume from the same pen on the other groups.

R. L.

THE FOOD OF PLANTS: An Introduction to Agricultural Chemistry. By A. P. Laurie, M.A., B.Sc. Pott 8vo. Pp. ix. and 77. With 15 Illustrations. London: Macmillan & Co., 1893. Price 1s.

A NEAT little primer, "written as an experimental introduction to Agricultural Chemistry for beginners," and embodying a number of simple experiments by which the principles of plant nourishment are put before the student.

The idea of the book is an excellent one, theory and practice being happily combined, but occasionally the author's statements are somewhat crude or even misleading, while a sentence ending with a preposition and the use of "and which" are not to be commended.

In Chapter I., the relation between the plant and water is well and simply illustrated, but it is wrong to speak of the young root as "covered with little branch roots or *hairs*," branch roots are one thing and hairs another, and to make the two synonymous is to inculcate error for some one else to eradicate. Again, we question the wisdom of teaching that the soil holds "the water just as a sponge does"; when one of the points to be impressed on the embryo

agriculturist is that the interstices of the soil must be filled with air, and not saturated with water like a sponge.

Chapter II. deals with food obtained by the plant from soil, and is excellent as far as it goes, the student being taught that soluble substances only can pass into the roots, and that there are certain substances in the soil, soluble in water, which are absorbed by the roots for food, but no reference is made to the greatly increased power of solution afforded by the acid reaction of the root-hairs, or the carbonic acid gas dissolved in rain-water.

The nature of the soil, the "substances of a leaf," the composition of the air, and the absorption of carbonaceous food therefrom, are well explained, though surely in the heading on p. 40, "The Seed obtains Food from the Air," we should read Seedling for Seed. In the seventh and last chapter, on the source of the nitrogen required by plants, the author is a little too crude and brief in his explanation of nitrification and the tubercles on the roots of leguminous plants, "full of minute creatures, to which the name of bacteria have (*sic*) been given." The two appendices "Notes on the Experiments," and "On the use of the balance," contain useful hints.

Mr. Laurie should have got a botanical friend to look over his proofs, and thus have avoided the serious errors which mar what might have been a capital little book.

PRÉCIS DE TÉRATOLOGIE, ANOMALIES ET MONSTRUOSITÉS CHEZ L'HOMME ET CHEZ LES ANIMAUX. Par L. Guinard, Chef des Travaux de Physiologie a l'Ecole vétérinaire de Lyon. Précedé d'une préface par M. le Dr. Camille Dareste. Pp. 512. With 272 figures. Paris: J. B. Baillière et Fils, 1893. Price 8 francs.

AFTER the great work of Is. Geoffroy-Saint-Hilaire, published from 1832–1837, for many years there was little done in Teratology. A few isolated papers contained descriptions of unusual monsters, but the attention of naturalists was so absorbed by the new light thrown on the domain of normal Nature that her freaks and sports were almost disregarded. Then, in quite recent years, came Camille Dareste's series of studies on the artificial production of monsters, and a number of other studies of which Mr. J. A. Thomson gave an interesting *résumé* in the last number of NATURAL SCIENCE.[1] The more recent part of the study of monsters concerns itself with the conditions of their production, and has been called by Dareste Teratogeny, as distinguished from Teratology, the record and classification of the types. The present volume is limited designedly to Teratology, and is meant to be a compact and convenient guide to the known types of abnormality; for, if it be, at the first glance, a paradox to write of types of abnormality, or to discuss the laws of disorder, a very brief study of M. Guinard's volume will convince that monsters are not the frolic of chance, but an inverted or tip-tilted regularity. Some are caused by mere mechanical pressure or torsion, and cause and effect tread on each other's heels as closely as the pressure of a boot and the growth of a corn; in others unusual condition, raising or lowering the temperature beyond the eurythermal limits, alteration of the natural position, conditions of strain (as on a centifrugal machine) tend to produce abnormalities, although the nexus of cause and effect is invisible; but, however produced, the productions fall readily into categories. M. Guinard gives, in a series of chapters, accounts of simple anomalies, classified according to the organs or

[1] NATURAL SCIENCE, vol. ii., pp. 294–306.

tissues chiefly affected; of complex anomalies, like hermaphroditism, apparent or real; and of monsters single or double. The illustrations are very useful, and as appropriately horrible as the subject demands. There is an index; full table of contents; and glossary of terms.

Altogether it is a most useful book, and is to be commended to all anatomists and embryologists. P. C. M.

THE EMBRYOLOGY AND METAMORPHOSIS OF THE MACROURA. By W. K. Brooks, Professor of Animal Morphology in the Johns Hopkins University, and F. H. Herrick, Professor of Biology in Adelbert College. National Academy of Sciences, vol. v., Fourth Memoir. Pp. 325–576, with 57 Plates.

IN this beautiful memoir the authors have made a notable contribution to knowledge of the development of the Macroura. Animals which, like the Crustacea, pass through a series of successive moults during their larval life, naturally exhibit a series of gradations leading towards the adult condition. Fritz Müller and Claus showed that in this group a very large number of orders, families, genera, and species display the relation between ontogeny and phylogeny. At the same time, Claus pointed out that an equally large number of adaptive larvæ are to be found, and in the introduction to this memoir Dr. Brooks strongly insists on the necessity of careful and exhaustive study in each case before the significance of a larval history can be understood. The survival of larvæ living free in water depends on their adaptation to their present environment, and so recapitulation of ancestral stages in the present metamorphosis must depend to a large extent on the persistency of those external conditions to which the larvæ were originally adapted.

The chief forms studied in this memoir are *Stenopus hispidus*, *Alpheus*, and *Gonodactylus chiragra*. In addition to anatomical and embryological detail, a number of side issues of general interest turn up. Thus one would expect (says Dr. Brooks) that the least specialised species are the most widely diffused. But the larvæ of *Stenopus hispidus*, although *Stenopus* is one of the most highly specialised of the Crustacea, are most widely distributed. Specimens from the Indian Ocean and the South Pacific agree with those from the West Indies down to the most minute marking. "It is well protected from enemies by a thorny armour of hooked spines which cover all the upper surface of its body and limbs, and, as all the hooks point forward, the attempt of an enemy to swallow a *Stenopus* must be difficult and painful."

Many of the species of *Alpheus* live as parasites in the canal of sponges, and individuals of the same species living in different species of sponges may differ greatly in colour and habits.

In *Alpheus* and in *Stenopus*, after impregnation takes place, the nucleus divides and a syncytium of eight nuclei is found. In *Alpheus* there is a slight invagination and a modified gastrula is found. But in all the accounts of development given readers will be struck by the want of distinctness in the separation and formation of the germinal layers. It certainly seems as if the familiar successive origins of the germinal layers are present only in a very vague manner in this group.

The account of the larvæ of *Gonodactylus chiragra* is specially valuable, as so little is known of the Stomatopoda. In the present work, Dr. Brooks is able to confirm most of the results he reached in the "Challenger" monograph.

CONTRIBUTIONS TO THE ANATOMY OF THE ANTHROPOID APES. By Frank E. Beddard, M.A., Prosector to the Zoological Society of London. *Trans. Zool. Soc.*, vol. xiii., part v., 1893 (pp. 177–218, plates xx.–xxviii.).

THERE will be few people who failed to hear at least of the existence of the celebrated chimpanzee "Sally"; her reputation, like that of some others, higher, perhaps, in the zoological scale, was made not entirely by her own unaided merits, but was largely due to the assistance of the Press. Her lamented death gave rise to obituary notices more extensive than those commonly devoted to archbishops. Human greatness, however, is apt to be but transitory; and apparently also the same is the case with anthropoid apes; but the waning reputation of "Sally" will, perhaps, be renewed by the above-quoted memoir upon her anatomy. "Sally" belonged to a species of Chimpanzee which was originally described by M. du Chaillu as *Troglodytes calvus*; the species was not by any means universally accepted at the time of its description; but the present paper sets at rest any doubts which may be still entertained upon the matter. The question of the species of Chimpanzees is one which is not yet decided, nor is it yet ripe for settlement; the material in the way of skulls and skeletons available in the museums of Europe is not sufficient. A great many different names have been at various times applied to supposed species; but at the present moment no more than two can be regarded as having been definitely proved; those are, of course, *Troglodytes niger* and the species which forms the subject of Mr. Beddard's memoir. This ape was remarkable not only for being the only individual of her kind ever exhibited at the Regent's Park menagerie, but also for her intelligence and long life. From the condition of the teeth it appears that she was about eleven years old; at any rate she lived in the Gardens for no less a period than eight years, which is, for an anthropoid, quite phenomenal. "Sally" finally yielded to a complication of diseases which rendered it impossible to give any account of the viscera; this is to be regretted, as the chances of studying the visceral anatomy of this species are not likely to be frequent. The absence of any account of the internal structure of the animal is, however, made up for by an account of the muscular anatomy and of the brain. The latter organ is well figured in several aspects; as might be expected, it does not show any great differences from that of the common Chimpanzee; nor is the muscular anatomy particularly characteristic; such points of difference as exist are dwelt upon by the author. The reader may be interested to hear that the brain is now to be seen in the Oxford University Museum; the skin has been stuffed and is in the Museum of the Hon. Walter Rothschild at Tring. A great feature of the paper, as is, indeed, usually the case with the excellent publications of the Zoological Society, is the illustrations; we do not intend to imply by this that the text in any way falls short of the illustrations; but it certainly is the case that the plates with which the publications of this Society are adorned are of their kind unsurpassed. We have in the present paper figures life-size of the full face, of the head seen from above (evidently to show its dolichocephalic character), of the hands and feet, and a whole plate and part of another is devoted to the brain. The second part of Mr. Beddard's paper deals with another anthropoid, who was familiarly known by the name "George"; this animal was thought to be a representative of the smaller Orang, *Simia morio*; Mr. Beddard, however, is not convinced of the correctness of this view, which was partly based upon the supposition (erroneous, as it now turns out) that the ape was elderly;

"George" appeared on an examination of the teeth to be after all a mere baby. A beautiful coloured lithograph of the head occupies one plate, and the hands and feet are also illustrated. It is, perhaps, unnecessary to say that the memoir is entirely technical in character, though the first few pages, occupied by some discussion of the species of Chimpanzees, will be found to be of somewhat more general interest.

THE BIRDS OF DERBYSHIRE. By F. B. Whitlock, with Notes by A. S. Hutchinson. 8vo. Pp. vi. and 235. Illustrated. London and Derby: Bemrose & Sons, 1893.

THE fact that no complete history of the bird-life of such an important and interesting county as Derby has hitherto appeared, affords ample justification for the issue of the little volume before us, in which the subject appears to be as fully treated as materials permit. The author calls attention, however, to the lack of local observers in the wilder districts of the country; and it may be hoped that the result of his labours will be to stir up other ornithologists to complete our knowledge of the subject. To show how necessary is local observation, it may be mentioned that, according to the author, till quite recently the merlin was considered to be only a casual winter visitor to the county, whereas it actually breeds on the high peak. It is to be regretted, however, that the efforts of the pestilent gamekeeper have, within the last year or two, almost, if not entirely, exterminated this falcon from its Derbyshire haunts. Of the five plates illustrating the work, four are views of scenery, while the fifth represents a remarkable variety of the corncrake.

Although the work is, of course, to a great extent of local interest, it contains many observations bearing on the subject of British ornithology in general, and must therefore be of value to all students of that science. R. L.

FOREST TITHES AND OTHER STUDIES FROM NATURE. By "A Son of the Marshes." 12mo. Pp. 208. London: Smith, Elder & Co., 1893.

THE tendency of modern science to become a mere record of "section-cutting" and of the dry details of comparative anatomy is so marked, that we have sometimes feared whether the old-fashioned field-naturalist was not in as much danger of extermination as many of the animals in which he took such a delight. Happily, however, the observer who writes under the *nom de plume* of "A Son of the Marshes" shows that the field-naturalist still exists among us in his best form; and his observations on the varied types of animal life that may still be seen within easy access of London affords us another example of the truth of the old story of "Eyes and no Eyes." Most of the essays in the little volume before us have already seen the light as separate articles, either in the *Times*, the *Cornhill*, or *Blackwood*, where many of our readers have, doubtless, ere this, perused them with pleasure. All who have done so, we think we may safely assert, will have still greater gratification in seeing them in their present guise, as it would have been a thousand pities had such delightful reading remained buried in the pages of a magazine or the columns of a newspaper. Whether in describing the otter, as he slinks alone the river-bank, with which his coat harmonises so closely in colour as to render him invisible to unpractised eyes, or in recording the movements of the

heron watching for trout at some broken weir, the author is equally at home; and we can thoroughly recommend his book to all lovers of Nature as a living picture of many British animals in their native haunts. R. L.

THE RECRUDESCENCE OF LEPROSY AND ITS CAUSATION. By William Tebb. 8vo. Pp. 412. London: Swan Sonnenschein, 1893. Price 6s.

THIS is a book written to show that vaccination is accountable for the spread of leprosy. We go so far with the author in entirely condemning the system of arm-to-arm vaccination, for it is definitely certain that though the health of the child may be satisfactory, in 99 cases out of 100 there is no means of ascertaining the condition of its progenitors. The author has collected together much information concerning leprosy that is valuable to the professional as well as interesting to the general reader, and concludes by observing that, as the disease is "practically incurable, it behoves all interested in the public well-being to do their best to prevent its diffusion," which he considers is largely due to the practice of vaccination.

EVOLUTION AND MAN'S PLACE IN NATURE. By Henry Calderwood, LL.D., F.R.S.E., Professor of Moral Philosophy, University of Edinburgh. Pp. 349. London: Macmillan & Co., 1893. Price 7s. 6d.

IN the minds of many some of the value of a book on Man's Place in Nature, written by the holder of an endowed chair of Moral Philosophy in a Scotch University, will be discounted from the outset. The Professor must hold a brief for his client, and his client is Man as a Moral Agent. The interest of the book or the lectures for such readers resolves itself into a curious contemplation of the byeways by which the author shall arrive at the known goal. The Scotch Professor of Moral Philosophy, at whose feet this reviewer had the duty of sitting, cleared a Stanley path through the forest of Science, raucously scaring the poor pigmies, shooting down the detestable niggers, hacking through the forest, marching straight with blundering and boisterous declamation on his inspired mission. Quite other is Professor Calderwood. On his way to his conclusions he dallies with variation and environment, embryology and evolution, very agreeably passes the time of day with Huxley and Hæckel, Eimer and Weismann, Helmholtz and Darwin, and says what very intelligent fellows (in a moderate and non-moral way) they all are. But when the real business comes on (in the last chapter) the naturalists rather fade away, and are replaced by Aristotle and Plato, Butler and Green, Shakespeare and Holy Writ, and Professor Ray Lankester; for Professor Ray Lankester has written on "Degeneration"—a branch of the theory of Evolution held in high honour among the dogmatic.

Professor Calderwood accepts the principle of evolution so far as man's body is concerned. With the proviso that life itself is inexplicable on mechanical or chemical grounds, and with the usual quotation from Huxley against abiogenesis, readily enough he concedes that there is an enormous preponderance of evidence in favour of the organism of man being of the same kind, and descended in the same way, as other organisms; but in addition to this organic life, he claims for man "rational life" unexplained by science and giving to man a dominance and position in nature totally unex-

plained by his physical nature. To arrive at this, he goes over a large field, where it would be impossible to follow him in detail, pointing out a multitude of distinctions—as, for instance, that man is a producer, animals only consumers, animals are modified by environment, man can modify environment, animals have sensibility, man rationality, and so forth. All these are points of discussion that, however Professor Calderwood trick them out in new settings, have been debated abundantly; and it is no unfair thing to say that one knows beforehand how the facts will appear to different men. Professor Calderwood and his kind exaggerate the mechanical rigidity of "instinct," dilate on the breaking down of animals in unaccustomed circumstances, and dwell on the glories of Shakespeare and Newton. The Darwinian and his kind dwell on the gradations by which "instinct" seems to pass into deliberated adaptation of means to ends, lavish adulation on the intelligence of the dog, and grovel before the animal stupidity of the savage.

Professor Calderwood, and with him many others, still fear what Carlyle called "the monkey damnification of mankind." They have lost the old fears now that they have lost the battle over the evolution of the body: they redouble them on the question of reason. To this reviewer it seems inexplicable that as all men know we have (to use the popular terms) reason, conscience, power of making moral judgments, and the apparent choice between them, it should matter whether these came suddenly or gradually; and it appears extraordinary that when all the processes of the world (including evolution) are a "sovereign wonder of superhuman fixedness of law," any writers should be so concerned to establish jerkiness in the law. Thus, Professor Calderwood with considerable pride, leads up to an argument for the existence of deity by coming to the conclusion that he has shown the incompetence of science to explain life, mind, and reason. Then he goes on: "There is a Power operating continually in Nature, which does not come within range of the observation possible for scientific modes and appliances, yet to which science is ever indirectly bearing witness. This Power has manifested itself at the most impressive periods in the world's history, first at the appearance of Organic Life, again on the appearance of Mind, and again on the advent of Rational Life." A smoothly-running process, an organic growth of the world of matter and life, a growth on which the fruits of life, of mind, and of soul appear in their due season, seems to Professor Calderwood less compatible with theology than an interrupted and repeatedly tinkered Nature.

This book is devoid of scientific value, and no doubt was not intended to have any; and while it cannot conceivably help to support the faith of anyone who has not in his mind a thought-tight partition between faith and reason, it may be a consolation to many good stupid people sorely tried in their faith.

THE YEAR BOOK OF SCIENCE. Edited for 1892 by Professor T. G. Bonney, D.Sc., LL.D., F.R.S. 8vo. Pp. 519. London: Cassell & Co., 1893. Price 7s. 6d.

IN this second volume of the Year Book of Science the unevenness of the different sections is still very noticeable, and the book might be greatly improved by stricter editing. We observe that the authors of the different parts take various views as to the objects of

this annual. One section or chapter may be all that it ought to be, and may give a good outline of the year's advance in some branch of science; but the next section is treated quite differently, and leaves us in entire ignorance of the advances made, except in some small field specially cultivated by the author.

As examples of good outlines we may mention the Mineralogy and Petrology, by G. T. Prior; Stratigraphical Geology, by H. B. Woodward; Palæontology (Vertebrate), by R. Lydekker; Palæontology (Invertebrate), by J. W. Gregory. Then comes a section on Palæobotany, by T. Hick, in which the whole of the prolific literature published during the year on fossil plants is ignored, except a few species from the Coal-measures, and a single Alga from the Permian. To this author the whole Tertiary and Secondary flora is non-existent!

The sections on Physical Geology and Geography, by H. G. Seeley; and Anthropology, by H. G. Seeley, are very imperfect. Biology (Animal), divided into several sections, and treated of by C. H. Fowler, C. S. Sherrington, R. I. Pocock, and R. Lydekker is much better.

Biology (Botanical) is divided between W. B. Hemsley, who takes Systematic and Geographical Botany; G. Massee, who writes Morphology and Biology of Plants; D. H. Scott, who treats of the Minute Anatomy of Plants; and F. E. Weiss, who abstracts the literature on the Physiology of Plants. But in some unaccountable way all these botanical writers have forgotten to mention Lubbock's large monograph on Seedlings! Each probably thought that it would be in another section, and so it was missed altogether.

M. MARTINUS NIJHOFF, of the Hague, has just issued a "Catalogue de Livres sur les Possessions Néerlandaises aux Indes Orientales et Occidentales sur l'Empire Indo-Britannique, les Possessions Espagnoles, Françaises, Portugaises, la Chine et le Japon, l'Australie." The catalogue is arranged under headings, and will be of assistance to those interested in these countries. Pp. 280.

THE Administrator's *Annual Report of British New Guinea from 1st July*, 1891, *to 30th June*, 1892, has just been issued as a Blue-book by the Colony of Queensland. The affairs of the Possession are reviewed under the separate heads of Legislation, Administration of Justice, Administrative Visits of Inspection, Government Property and Works, Establishments, Meteorology, Trade, Mission Work, Lands, Prisons, Finance, Native Dialects, Scientific Contributions, and Reports by Officers. It contains 113 pages of text and 9 maps. No one can read this official document without being impressed with the extraordinary amount of information it contains of the highest scientific value. This young colony is more favoured than most in having an officer in charge who is not only a highly efficient Administrator, keenly alive to all the best interests of the natives. but a man possessed of an unusually wide range of attainments, Under the head of Administrative Visits of Inspection, the dispatches contain an amount of geographical, geological, zoological, anthropological, and linguistic data, collected chiefly by the Administrator himself, of unusually high value. The Report contains also sub-reports by officers specially commissioned for the purpose, *i.e.*, by Mr. Moreton on his unsuccessful Expedition from Philipps Harbour towards

Mount Suckling; on Meteorology, by Mr. Clement Wragge, Government Meteorologist of Queensland, and a long and valuable report on the Geology of the Possession by Mr. Gibb Maitland, of the Queensland Geological Survey.

Meteorological observations are taken at four stations in the Possession, at the capital, at Mekeo, and at the headquarters of the Eastern and Western districts. "Unfortunately, complete records can hardly be procured, except at Port Moresby, owing to the fact that officers at other stations are often necessarily absent from their headquarters on other duties." At Port Moresby the mean atmospheric pressure for the year 1891, at 9 a.m., was 29·919 inches. The grand mean temperature was 83·0 F.—the highest shade (2nd February) 96·2°, the lowest (27th August) 72°. The rainfall for the year was 72 inches, of which 53 inches fell in the first five months of the year. It rained on 95 days. At Samarai (in China Straits), from April, 1891, to March, 1892, 126·5 inches fell on 174 days. During the three months of July, August, and September a fall of 59 inches was recorded there, while only 15 inches fell at Port Moresby. Dufaure Island is the meeting place of the Port Moresby and Samarai climates. Sir W. Macgregor contributes, as an appendix, a vocabulary of the Kiriwina dialect in the Trobriand Islands, with observations on its grammar, and the Rev. W. Bromilow one on the aboriginal vocabulary of Dobu.

In concluding his Report, which, we repeat, is of great scientific value, the Administrator remarks: "The results of native administration appear, on the whole, fairly satisfactory for the year. In not a few districts natives are settling down to more systematic work in preparing exports for the trader. . . . In some of the districts where native murders were fearfully common two or three years ago, this crime has completely disappeared, or become very rare. No doubt what in these days may be called 'much time' is required to acquaint the Papuan tribes with the working of the Government, and the impartial and beneficial action of law. But they possess an aptitude for such tuition which cannot but appear to be extraordinary to any person acquainted with the history of civilisation among the present existing cultured races of our globe. If the Papuans are allowed anything like reasonable time—a time which compared to that required for such a purpose by other races would be a very short period—there can be no doubt that they will become a very considerable, if not an important, unit in the Australasian dominions of the Queen." These observations strikingly confirm the opinion of the natives formed by Mr. H. O. Forbes during his intercourse with them, even before Government supervision had begun to take effect, and expressed in *Blackwood's Magazine* for July, 1892.

The English Catalogue of Books for 1892 has just appeared (Sampson Low, five shillings). We are glad to see that the plan of arrangement adopted last year is adhered to; there is no system more convenient for a reference book than the purely alphabetical. We would suggest, however, one improvement, which is that all the miscellaneous collection at present under "Transactions" should fall into their proper alphabetical position in the next issue. "Transactions" is clearly wrong, "Serials" or "Academies" if you will, but not "Transactions."

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. JOHN STORRIE has resigned the Curatorship of the Cardiff Museum.

DR. JORDAN has arrived at Tring from Hildesheim, and will take charge of the Entomological collections belonging to the Hon. Walter Rothschild.

MR. GEORGE MASSEE has been appointed to succeed Dr. M. C. Cooke in the Kew Herbarium. The second volume of his *British Fungus Flora*, dealing with Agaricineæ, has appeared.

MR. F. A. BATHER, of the British Museum of Natural History, has obtained special leave of absence on account of a weakness of his eyesight. Mr. Bather will spend the greater part of four months on the sea and will visit Teneriffe, Cape Town, Hobart Town, Sydney, Japan, San Francisco, Burlington, and New York. He left on March 30.

MR. A. SMITH WOODWARD is on his way back from the Lebanon, where he had gone to study the important deposits of fossil fish of Cretaceous age.

THE funds of the McGill University have been increased in the sum of 160,000 dols., the gift of Mr. Molson and Mr. Donald Smith.

THE syllabus of work for the "Edinburgh Summer Meeting" for the seventh Session, July 31—August 26, 1893, has just been issued. The business of Section C (Natural Science) consists of ten lectures on Comparative Psychology, by Professor Lloyd Morgan; Hygiene, ten lectures by Dr. Louis Irvine; Biology, twenty lectures by J. Arthur Thomsom and Norman Wyld; Practical Botany (including Field Work), twenty meetings, Robert Turnbull; Field Geology, ten excursions directed by Norman Wyld; Practical Zoology (at Granton Marine Station), twenty meetings, J. Arthur Thomson; Regional Survey of Edinburgh and neighbourhood, twelve lectures.

THERE will also be delivered twenty lectures on Contemporary Social Evolution by Professor Geddes, and many other courses on various branches of Education, such as History, Social Science, Music, Elocution, &c. Particulars can be obtained by application to J. Arthur Thomson, M.A., University Hall, Edinburgh.

WE are glad to observe that a suitable inscription has been recently placed on the pedestal of the bust of Sir Richard Owen, which has a place of honour in the Pavilion of the Palæontological Gallery at the Natural History Museum. This bust is a plaster cast of the Hamo Thornycroft marble (1880), preserved at Sheen Lodge, and was presented to the Museum by the family about four years ago. The inscription reads as follows:—

SIR RICHARD OWEN,
K.C.B., M.D., D.C.L., LL.D., F.R.S., &c., &c.
Superintendent of the
Natural History Departments
of the British Museum,
from 1856—1883.

Born, 20th July, 1804. Died, 18th Dec., 1892.

"Accesserunt ossa ad ossa, unumquodque ad juncturam suam."—
Ezech., xxxvii., 7.

We are informed by Dr. Henry Woodward that the appropriate quotation from the Vulgate was selected by the Rev. Professor Bonney.

THE fourth annual meeting of the Museums' Association, which is to be held in London under the Presidency of Sir W. H. Flower, K.C.B., will commence on Monday, July 3, and probably extend over four or five days. The mornings will be devoted to the reading and discussion of papers, but the afternoons will be given up to the inspection of various metropolitan museums, under competent leadership.

A GEOLOGICAL Society has recently been organised in Washington for the presentation and discussion of topics of interest to geologists. The constitution and standing rules were subscribed to by 109 founders at the first public meeting, March 8, 1893. Its members are of two classes, active and corresponding. The annual subscriptions of the first are 2 dols., and of the second 1 dol. Meetings will be held on the second, and generally also on the fourth Wednesday of each month, from October to May inclusive. The journals and bulletins of the various societies appear to furnish sufficient opportunity for the publication of papers read before the Society, so that for the present the Society will not undertake to publish the papers presented. It will probably issue one bulletin each year containing the address of the retiring President, and such other matter as the Council directs. The officers, elected February 25, 1893, are C. D. Walcott (President), S. F. Emmons and W. H. Holmes (Vice-Presidents), A. Hague (Treasurer), Whitman Cross and J. S. Diller (Secretaries); the Council being G. F. Becker, T. M. Chatard, G. H. Eldridge, G. K. Gilbert, G. P. Merrill. We should like to have seen the organisation of local excursions among their objects, for field observation is an essential to the increase and diffusion of geological knowledge.

THE collection of Lepidoptera (chiefly Micro-Lepidoptera) formed by the late H. T. Stainton has been presented by his widow to the Natural History Museum. The presentation is the more valuable as it includes the original drawings and papers illustrative of the specimens.

THE Zoological Society of London has just issued its Annual Report of Accounts for 1892. We are glad to note a slight improvement in gate-money (£272) over 1891, which points to an increased appreciation in the efforts made by the Society to render the Gardens interesting to the public. It is curious to see a falling-off in the elephant rides of £3 19s. 4d., but still the comfortable amount of

£606 was received from this source during the year. There is a considerable improvement in the admission fees, which points to an encouraging influx of new Fellows. Turning to the payments, we find that out of an expenditure of £25,968 no less than £17,618 was expended on the Gardens, an amount which speaks eloquently for the excellent standard maintained. Of this amount, £843 was spent on animals and their transport, £3,974 on food, £3,468 on salaries and pensions, £765 on horticulture and garden-work, £2,628 on works and buildings, and £3,466 on menagerie expenses. We have commented on the expense of the *Zoological Record* on page 324.

Mr. Edward Best, who has been on the staff of the Geological Survey for 38 years, has now retired. His place as acting secretary is taken by Mr. W. Topley, and Mr. A. C. G. Cameron has been promoted to the rank of geologist.

At the annual meeting of the Norfolk and Norwich Naturalists' Society, held at the Norwich Museum on March 28, Mr. Thomas Southwell was elected president. The address of the outgoing president, Mr. H. B. Woodward, was devoted to various local topics, principally geological.

The work of remounting the great series of microscopic preparations made by the late Professor de Bary, and acquired by the British Museum, is nearly completed. The slides illustrating the comparative anatomy of the vascular plants, and those dealing with the structure and life-history of the lower forms, have been successfully restored, and the Fungi, which have been left to the last, are three-parts done. The value to students of Botany of the presence of this great collection in this country can only be indicated, since it will always remain as the authentic basis of the great botanist's work.

Professor Herdman sends us the "Sixth Annual Report of the Liverpool Marine Biology Committee." The laboratory at Port Erin appears to be a flourishing institution, and a considerable amount of work has already been turned out. The Committee have secured the services of Mr. Henry Vanstone, of the Royal College of Science, South Kensington, as "resident Curator," and important additions have been made during the winter to the laboratory. A great number of students appear, from what we have heard, to be availing themselves of the opportunities of working there.

We regret to learn that Mr. T. D. A. Cockerell has found the climate of Kingston, Jamaica, too trying for his health, and has been obliged to leave the Museum there and return to his old home near the Rocky Mountains. Mr. Cockerell was doing some good work in economic entomology, and his departure will be a great disappointment to him and a distinct loss to the Kingston Museum. His address will be Las Cruces, New Mexico, U.S.A.

Mr. E. J. Bles has been appointed Director of the Laboratory at the Plymouth Marine Biological Station.

We hear that Professor Albert Gaudry has resigned the position of Professor of Palæontology at the Jardin des Plantes.

OBITUARY.

ALPHONSE LOUIS PIERRE PYRAMUS DE CANDOLLE.

BORN OCTOBER 28, 1806. DIED APRIL 4, 1893.

THE great Swiss botanist died at Geneva, in the old house in the Cour St. Pierre, which, built by one of two brothers who took refuge in Geneva at the Reformation, had been the home of the family for many generations.

He was born in Paris a few days after his father had been rejected on his second presentation to the Institute in favour of another botanist, much his inferior, Palissot de Beauvois, and in his autobiography published by his son in 1862 ("Mémoirs et Souvenirs"), Auguste speaks of the consolation he derived from the happy event. Two years after, Auguste went to Montpellier, where he succeeded Broussonet as Professor of Botany, but in 1816, owing to political troubles then rife in France, he resolved to return to his native town, at the University of which a chair in natural history was founded expressly for him. This he held till 1834, when he resigned, his place being taken by his son, Alphonse, the subject of the present memoir.

The name of De Candolle would always be an honoured one among botanists, even if Auguste had not had in Alphonse a son to carry on his great work and strike out new lines of his own, while, now the son is gone, Casimir, the grandson, a well-known botanist, lives to perpetuate its fame in the third generation.

Every botanist who has any knowledge of systematic botany, every gardener who lays any claim to scientific knowledge, knows and has proved the grand *Prodromus Systematis Naturalis Regni Vegetabilis* which, begun in 1824 by Auguste and finished in 1873 by Alphonse, will be a lasting memorial of their genius and perseverance. There was grit in these old botanists.

Nowadays when a man scores a paper in a few weeks or months, or perhaps the best part of a year, one looks back with admiration at Auguste de Candolle, no longer young, starting, in 1816, to monograph all the Orders of the vegetable kingdom according to the then but little known natural method. When, after five years of hard work, only eleven Natural Orders were complete, including, nevertheless, two of the most important—Ranunculaceæ and Cruciferæ—he

realised that the work was too vast, and started on the *Prodromus*, evidently, from its name, intended as an abridged form of the larger work. After the first few volumes, however, the *Prodromus* became itself a series of complete monographs, thus ultimately realising Auguste's original ideal. The first volume appeared in 1824, and the name of the father alone appears on the title-page of this and the six that follow. In his historical account in the seventeenth and last volume, Alphonse says his father was almost entirely responsible for the matter of the first seven volumes, at which he worked unceasingly for twenty years, from 1822 till his death in 1841. From the third volume onwards, however, he was assisted by his son, who, after his father's death, carried on the work for thirty-two years more, adding ten volumes. Though he received a great deal of help from other botanists, of whom he gives a list, with the number of pages due to each, he was solely responsible for the editing, and also worked up twenty-six of the monographs.

Folks may wonder, he says, why, with so many helpers, the work took so long. But monographers find their difficulties increased with the number of species, specimens, characters, and synonyms. Moreover, rarely are herbaria and books to be had at one and the same time without some hindrance or delay, libraries being often separated from the plants, or wanting in the more recent works. Here a most devoted botanist lacks either the books or plants, while there the botanist is himself wanting in zeal, method, or time for work. There are also authors who, having undertaken a monograph, do not forward it in due time, or even give it up altogether. This last failing in particular caused loss of time, and necessitated frequent division of the volumes into sections and transposition of the Orders, one (Artocarpeæ) having, at the last moment, to be abandoned, and, finally, the conclusion of the work, at the end of the dicotyledons, "ne tertiam botanicorum generationem occideret!" In the "Monographiæ Phanerogamarum," however, the number of missing Orders is gradually being reduced, seven volumes having been issued at the time of his death, while others are in active preparation.

The *Prodromus* includes 214 natural orders, 5,134 genera, and 58,975 species; 15 per cent. of the genera and 25 per cent. of the species are new. Of the 13,194 pages, the de Candolles (including Casimir, who supplied 260) wrote 5,950 pages, various curators of the Candollean Herbarium 1,475, and other botanists, among whom are three Englishmen—Bentham, Sir J. Hooker, and A. H. Weddell —5,769.

But the *Prodromus* was not the sole outcome of all these years. Until 1850 Alphonse occupied the Chair, instituted for his father, at the University of his native town, but in this year resigned it, in consequence of the state of political affairs in Geneva. In 1835 he had published an "Introduction to the Study of Botany," a book of two volumes, and in 1844 produced a third edition of his father's

"Théorie Elémentaire." In 1855 appeared his "Géographie botanique raisonnée," published in two volumes, and containing, in 1,300 pages, "an exposition of the principal facts and laws concerning the geographical distribution of plants at the present day." "In its aggregation of facts and results, and in their skilful marshalling," says a reviewer in the current issue of the *Gardener's Chronicle*, "it is comparable only with the works of Darwin, some of whose views were indeed something more than foreshadowed by the great Swiss botanist." In 1867 he laid before the International Congress of Botanists at Paris his "Lois de la nomenclature botanique," which still forms the basis of modern botanical nomenclature. In 1880 appeared "La Phytographie," or the art of describing plants, a work of the utmost value to every systematist, and concluding with an invaluable list of the principal herbaria. His "Histoire des Sciences et des Savants depuis deux siècles" (1873) is a charming book, and quite intelligible to the general reader, while his "Origine des Plantes Cultivées," a translation of which is published in the "International Science Series," is also well-known.

These do not represent nearly all of the stately and courteous Professor's contributions to botanical science, but this is but a short biographical notice, not a bibliography, and no place can be found for his numerous papers dealing with very various aspects of botany.

De Candolle made several memorable visits to this country. When about two-and-twenty he came over to London, and leaving the metropolis in May went down to the West of England. Thence he worked up through Wales and the English Lakes to Glasgow, where he met Sir William Hooker, then Professor of Botany at the University. From Glasgow he went to Skye, and having done the island on foot, crossed to Inverness, and so back to London, where he arrived again in the autumn. Certainly a very creditable tour, considering the means of communication more than sixty years ago.

In 1866 he presided over the London Botanical Congress held at South Kensington, and in his address emphasised the great importance of an intimate relation between botany and horticulture. In May, 1850, he was elected a foreign member of the Linnean Society, of which he was at his death the senior foreign member.

In 1889 the Society conferred on him its gold medal, the highest honour it has to give. The President, when announcing the sad news at the meeting on the evening of April 6, mentioned that his son Casimir, who has done good botanical work, had been selected by the Council for nomination as a foreign member.

De Candolle loved the English and English ways, dining in English fashion and speaking English largely at home, and, in common with the rest of his family, was by no means friendly disposed to the political powers of his native city.

He was well and vigorous till within six months of his death,

when he began to feel old. His wife died some years ago, but he leaves two sons, to one of whom, Casimir, we have already referred as a well-known botanist.

WE regret to announce the death of the Rev. T. Wolle, pastor of the Moravian Church at Bethlehem, Penn. He was the author of books on the *Fresh-water Algæ of the United States* and on the *Desmids of the United States*. His work has value as that of a systematic pioneer and recorder of forms—a labour to which he brought great zeal and enthusiasm.

PROFESSOR Giuseppe Antonio Pasquale, who died at Naples on February 14, was Professor of Botany at the University, and Director of the Botanic Gardens. He was the joint author of a "Flora medica della Provincia di Napoli," published in 1841, and a "Compendio di Botanica," which reached a third edition in 1890. He also wrote an account of the "Flora Vesuviana," and other systematic and morphological papers, and compiled a "Catalogo del real orto botanico di Napoli."

CORRESPONDENCE.

I HAVE to thank Dr. Hurst for bringing to my notice his interesting suggestion as to how inoculation may be supposed to confer immunity from disease. The idea that the attenuating process produces, by selection, a new strain of microbe having a different physiological action, seems worthy of careful examination. Such a gradual production of a more slowly-increasing variety well explains the diminishing intensity of the disease produced by successive inoculations. As Dr. Hurst points out, the diapedesis of the leucocytes is soonest able to overcome such feebly-increasing organisms. There is, however, to my mind, this difficulty. Attenuating processes consist generally in growing under *unfavourable* conditions, and I cannot see why such conditions should favour the slowly-increasing rather than the rapidly-increasing varieties of the microbe. Indeed, it seems to me probable that, other things being equal, unfavourable conditions generally would rather select the more quickly-increasing. Quick breeding is such an obvious advantage under adverse circumstances, as affording, in each generation, more numerous chances of survival. There remains, of course, the suggestion that the selection may "act in some other way."

Supposing, then, we have a virus attenuated in the manner suggested by Dr. Hurst, how can immunity conferred by inoculation with the same be explained? We may take it as an established fact that the local inflammation attending inoculation is accompanied by diapedesis of the leucocytes. These leucocytes, in their abnormal state of activity consequent on inoculation,—and, perhaps, as Dr. Hurst suggests, in increased numbers,—and, after their victory over the feebler army of microbes, might be ready to attack and subdue the more formidable foe. This would explain momentary immunity, but I cannot see how such a state of

abnormal activity—which is inflammation—can be supposed to continue for months or years. Nor does it seem to me probable that an increased rate of production of the leucocytes within the body could be rendered permanent by the temporary action of the attenuated virus; for it is to be remembered that the immunity is supposed to extend over certain prolonged intervals—nine months in splenic fever, according to Pasteur, and seven years for the small-pox, according to the medical faculty; and if protective inoculation merely consists in the increase in the strength and activity of the standing army of leucocytes, then *one* inoculation should suffice for *all diseases*; he who is protected by inoculation with the cholera virus should be proof against small-pox, and the virus of scarlet fever should protect against typhus. Moreover, might not such a state of leucocytal activity be produced by other means without the introduction of microbes or poison from a diseased animal—an idea which, to say the least, is lacking in poetry? That such were the case is, indeed, a "consummation devoutly to be wished" and prayed for. If Dr. Hurst can prove his proposition he will have the thanks of a long-suffering public, threatened with the nightmare anticipations of a life spent in being inoculated—a veritable flying from "the ills we have to others that we know not of."

Finally, I must remind Dr. Hurst that the inoculation liquids of many experimenters contain no microbes. Dr. Koch's consumption vaccine, I believe, contained none, nor does that of Dr. Krafikine.

G. W. BULMAN.

"TWINS AND TRIPLETS" IN *Doris*.

MR. J. ARTHUR THOMSON refers to the frequency of "twins and triplets" in *Doris*, meaning thereby two or more embryos developing within a single egg, and suggests that it may be due to "the fact that the ribbons [of eggs] are battered to and fro by the surge."

I have observed the same phenomenon, however, in eggs laid by a *Doris* in an aquarium, and not exposed to any movement at all.

April 15, 1893. C. HERBERT HURST.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 5 JOHN STREET, BEDFORD ROW, LONDON, W.C.

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ERRATA.

P. 254, line 21, for "Caprettæ" read "Caprellæ."
P. 255, line 13 from bottom, for "Hasting" read "Harting."
P. 259, the block was lent by Dr. Sclater.